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THE ORIGIN AND RELATIONSHIPS OF THE ARAUCARIANS. I

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Perhaps I can make the purpose of this paper clear in no better way than by quoting a sentence or two from a recent paper by JEFFREY (**42**):

> The comparative, developmental, paleobotanical, and experimental investigation of the Coniferales is likely to throw more light on the stable and sound general principles of biology than that of any other large group of animals or plants, on account of their great geological age and remarkably continuous display, both as regards external form and internal structure in the strata of the earth. . . . .
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> It is finally clear that morphologists will find it necessary in the future more and more to adopt certain general working principles, as for example in the sister sciences of chemistry and physics. If there prove on trial to be no generally applicable fundamental principles in morphology, that branch of biological science cannot be too soon cast into the outer darkness, which prevails outside the scientific view of nature.

It is doubtful whether anyone is likely to dispute the truth of the first sentence quoted above. At any rate, it is sufficiently accurate to make an examination of the various opinions concerning the origin and relationships of the araucarians well worth while. It is a conspicuous fact that among those most familiar with the facts now known regarding conifers, the most diverse opinions are strongly held. It is further to be observed that these various opinions are grounded on the different values set upon different

classes of facts. The object of this paper is to attempt to set forth the principal views and to show on what sort of evidence each rests. Having gotten at the facts, we may then inquire into the relative merits of each sort of evidence and attempt to evaluate them. If such an evaluation could be made, we might rescue the subject of morphology from the fate of being "cast into outer darkness." I am very little inclined to think that it will be possible for any one investigator to set forth the evidence in a manner to satisfy the adherents of all views, but my purpose will be served if I shall induce those more competent to take up the subject with the serious intention of determining just what principles are generally applicable, if there are any, and what are the limitations to which various sorts of evidence are subject.

In a broad way the opinions regarding the origin of the araucarians may be grouped under three heads: (1) the lycopod theory, (2) the cordaitean theory, (3) the abietinean theory. In setting forth these various theories I shall attempt to present the salient facts and opinions of a number of those who have written on the subject, but no attempt will be made to present exhaustively the facts or even the opinions of any one. It will undoubtedly happen that in choosing what seem to me the strongest evidences for the different views and the strongest objections to them, I shall be unable to escape the effects of personal bias, and still less able to choose just as the authors themselves would choose. These are objections that cannot be wholly avoided. The best I can do is to be as impartial as possible and to beg indulgence in advance for the slips that are sure to occur.

The lycopod theory

The external resemblance of certain conifers to the lycopods has attracted notice from early times. The first serious attempt that I have found to set forth the evidence for believing that Coniferales, or at least certain of them, have originated from Lycopodiales is that of POTONIÉ (**47**) in 1899. A rather free condensation of his statement of this theory and the facts on which it is based is as follows: (1) conifers, except possibly Taxaceae, are derived from "Lepidophyten"; (2) the ancestor of a cone scale was a leaf that

was at once foliar and spore bearing; (3) this foliar organ became differentiated into two parts, (*a*) a reduced spore-bearing part and (*b*) a foliar part, somewhat after the fashion of an *Ophioglossum* leaf; (4) these compound organs were aggregated into cones; (5) in the process of cone formation the foliar portion was gradually reduced and the ovuliferous (spore-bearing) portion was increased; (6) in the pine cone the spore-bearing portion is the ovuliferous scale, in *Araucaria* it is the ligule, and in many other plants ligule-like outgrowths are to be explained in the same manner (**47**, pp. 320–323). He offers the following arguments in its support: (1) it harmonizes a great variety of facts; (2) this gradual development of the ovuliferous scale and reduction of the bract is in accord with geological history, because it is only in the Abietineae that the distinction between scale and bract is comprehensible, and they are later and more specialized forms than the earlier Araucariae and Taxodiae; (3) the brachyblast theory requires us to think of the ancestral forms as having less compact strobili and with cone scales more like a leafy shoot, and this is directly opposite to the historical fact. We shall return in a later paragraph to the question of how much of this evidence is still pertinent after the presentation of the more recent views.

In 1905 CAMPBELL (**8**) stated his opinion that "as both the pollen tube and seed formation are but further developments of heterospory, it is quite conceivable that these might have arisen independently more than once. The close resemblance between the conifers and the lycopods, especially *Selaginella*, probably points to a real relationship. The strobiloid arrangement of the sporophylls, as well as the development of the prothallium and embryo, are extraordinarily similar, and it is not unreasonable to suppose that this is something more than accidental. The strong resemblance between the method of the secondary thickening of the stem in the arborescent fossil Lycopodineae, and that of the conifers, as well as the anatomy of the leaves, suggests a real affinity. It is known that some of these bore seeds, which in structure and position may very well be compared to those of typical conifers." He reaffirmed this opinion in less precise terms in 1911 (**9**).

In 1905 SEWARD and FORD (**54**) surveyed the work upon araucarians up to that time, added a considerable body of observations of their own, and discussed thoroughly the bearing of all the available facts. They came to the conclusion that the Araucariae occupy a place so remote from other conifers that they should be set off as a separate order under the name of the Araucariales. It is difficult to summarize so extensive a paper, in itself more or less a review, within the limits necessarily set on such a paper as this. However, we may summarize briefly their conclusions and indicate more or less inadequately the nature of the evidence on which they are based. So far as their discussion of affinities is concerned, the argument may be divided into three parts: (1) Araucarineae are primitive plants; (2) there are numerous grave objections to the assumption that they have originated from the Cordaitales; (3) there are significant resemblances to various living and extinct lycopods.

1. *The Araucarineae are primitive plants.*—This thesis these authors attempt to prove in two ways. They show from a review of the fossils that have been assigned to this family that it is extremely probable that fossil stems are known as far back as the Permian, and possibly the Carboniferous, both as impressions and as petrifactions, that find their closest resemblances and affinities with the present-day araucarians. Cone scales resembling those of the EUTACTA section of the genus *Araucaria* are known far back in the Jurassic, farther back in fact than those of the Abietineae. Historically, then, they argue the Araucarineae are primitive plants. To the support of their argument from the geological record they bring the testimony of comparative anatomy and morphology. They think the primitive character of the group is indicated by the gradual transition of foliage leaves to cone scales or sporophylls, by the resemblance of the two cones in some species, by the simplicity (as they attempt to prove) of the ovulate cone, by the persistent leaf traces, by the anatomical character of the leaves, by the homogeneous character of the wood without resin canals or wood parenchyma, by the multicellular male gametophyte, by the lateral distribution of the numerous archegonia, and by the similarity of the embryo cap to a root cap.

2. *A cordaitean connection does not seem probable.*—The chief arguments for this view they summarize under three heads: (*a*) the presence of hexagonal contiguous pits on the tracheal walls, (*b*) a comparatively wide transition zone from protoxylem to secondary wood, (*c*) a resemblance in the form of the leaf and in the general habit of the vegetative shoots. They are of the opinion that the first two resemblances are of little significance because of their common occurrence among paleozoic plants. They are primitive characters, but do not necessarily indicate a relationship to Cordaitales. The external form of the leaves is not specially significant, for the internal structure does not indicate a close affinity with Cordaitales, and furthermore the more ancient araucarian leaves are less like those of Cordaitales than those of certain modern species.

Having refuted the supposed arguments for the cordaitean connection, they offer certain other objections for good measure. They point out that the leaf trace of Cordaitales is double when it leaves the primary wood, while that of the Araucarineae is single. They cite several other investigators in support of their opinion that the ovulate cone is simple in structure. They recognize that a comparison can be made between the simple appearing cone scales of *Araucaria* and the apparently double structures of the Abietineae. The evidences for the duplicity of *Araucaria* must be derived second hand from the latter. The evidence for their double nature rests on the assumption that certain abnormalities, in which an ovulate scale is replaced by a foliar shoot, indicate that the cone has been derived by condensation of a branching leafy shoot whose leaves bore ovules abaxially. While this may be probable enough (they do not even commit themselves to so much) for this group, it is not considered valid evidence in the interpretation of the apparently simple cone of *Araucaria*, which they believe to be an older type and more likely to exhibit primitive structures than the Abietineae. Their contention is that in the Araucarineae there is nothing to explain. The cone is just what it appears to be. The inverted vascular supply of the ovule is a normal feature of such bundles, and the duplicity is no more than a sort of ligular excrescence, such as is common among the Lycopodiales.

The resemblance of the male gametophyte to that of Cordaitales is no more than a parallel carried over by both from the pteridophytes. In short, there are not very many resemblances between *Araucaria* and *Cordaites*, and even these are no more than an expression of a common heritage from the pteridophytic ancestors of both. From all this SEWARD and FORD conclude that the Araucarineae are too unlike Cordaitales in too many respects to make the supposition of actual relationship probable. This conclusion leads logically up to the presentation of what they believe to be evidences of real affinity with the Lycopodiales.

3. *The significant resemblances of araucarians and lycopods* appear to these authors to be numerous and important. They recognize, however, that the application of JEFFREY'S (**27, 28, 29**) well known and widely accepted division of the vascular plants into two great divisions, the Lycopsida and Pteropsida, on the ground of the presence in the latter of leaf gaps and their absence in the former, would constitute a serious objection to a derivation of any conifers from a lycopsid ancestry. They prefer rather to question the validity of his generalization, and express the opinion that the characters on which it is founded "have been estimated at too high a value as indices of affinity."

Having thus cleared the ground by attempting to show that the Araucarineae are primitive, that it is unlikely that they have been derived from the Cordaitales, and that their possession of leaf gaps does not preclude a lycopsid ancestry, they reach the really critical portion of the argument. Direct comparisons are instituted with a number of living and fossil lycopods.

In the main body of the paper they have shown to their own satisfaction that the araucarian cone is simple in structure and so "poles asunder" from that of the Abietineae or Cordaitales. This suggests the direct comparison with the ovulate cone of *Lepidocarpon*. In opposition to the view of the discoverer (**51**), they are of the opinion that it may "constitute a (possible) connecting link between the Araucariae and lycopods" and approvingly cite SCOTT'S (**53**) admission that it furnishes an excellent argument for their view. The organization of the cone, the simple sporophylls, the single median ovule, and the ligule appear to present

so close a homology as to afford strong indications of real affinity. The method of attachment of the single megasporangium by a stalk and the similar cone scales serve to form the basis of a comparison with *Spencerites* (**49**).

The resemblance of the microsporophylls to those of *Equisetum* is noticed, but is not held to indicate a direct relationship. Though they admit that the typical lycopod did not have its microsporangia on the dorsal side of the microsporophyll, they think they see a certain significance in the fact that the *Araucaria* type is found in *Cheirostrobus* (**50**), a genus that has been thought to be one of those generalized types which serve as finger-posts to the paths which evolution has followed, and which is considered to be intermediate between club mosses and horsetails (**54**).

These investigators assert that not only are the ovulate cones simpler and more primitive in structure than those of other conifers and more like those of paleozoic lycopods, but they are more nearly like those fossils from the Mesozoic than are those of any other group. This accords with their view; whereas, if they had been derived directly from Cordaitales or indirectly through the Abietineae, they should show some approach to the supposed leafy shoot predicated by the brachyblast theory, as the fossil history is followed backward toward the ancestral forms. They are of the opinion that no such transitional forms are known. On the contrary, they are of the opinion that the older forms show an approach to the lycopod situation in having smaller leaves and cone scales, with a gradual transition between the two organs.

They point out that the stem apex more closely resembles that of lycopods than that of ferns, though they do not attach much value to this fact. The exarch veins of the leaf may be regarded, they think, as a possible "ancestral feature which has disappeared from the vegetative stems." The leaf traces are accompanied by a group of cells in the cortex which the authors compare to the "parichnos" in lepidodendroid stems.

While they admit that there are many points of dissimilarity between araucarian stems and those of the lycopods, they do not think that any of them constitute an insuperable barrier to the derivation of the one from the other. They point out that the

scalariform tracheids of the lepidodendroid stems is paralleled by the same structures in the transitional primary wood of *Araucaria*. They do not urge this point as a strong argument, but merely point out that the presence of the two distinct types of pitting in the mature secondary wood of the two groups does not "necessarily imply separate lines of descent." The resin canals of the araucarians can be derived as well from the mucilage cells and canals of the Lepidodendreae as from those of the Cycadofilicales.

They are strongly of the opinion that the multicellular pollen grains of the araucarian alliance are very different from those of Cordaitales or any other recent seed plants. They differ from the former in the arrangement of the cells, and from the latter in the much greater development of the vegetative cells. A comparison with the microspore of *Selaginella* or *Isoetes* appears more convincing. The reduction they "connect with the substitution of siphonogamous for zoidogamous fertilization, which would demand as much space and material as possible for the production of the pollen tube."

STILES (**61**) argues that the conifers can be derived more readily from the lycopods than from the Cordaitales. His argument is divisible into two parts: (1) an attempt to show that Podocarpeae (and hence other conifers, for he holds that all have had a common origin) cannot have been derived from Cordaitales and must, therefore, have been derived from the only other(?) available source, the lycopods; and (2) a direct comparison of conifers and lycopods to show the possibility of deriving the former from the latter.

Under the first head he adduces much excellent evidence to show that the podocarps are closely related to the araucarians. He also attempts to show that the conifers are monophyletic. The next step is to show that podocarps cannot have been derived from Cordaitales, in consequence of which the other conifers are likewise excluded from such an origin. He enumerates four points which he considers sufficient to preclude the possibility of the primitive podocarps having originated from Cordaiteans: (*a*) the stem of these podocarps is no more like that of the Cordaitales than it is like

that of conifers in general; they may have derived certain resemblances from a common ancestry but are not on that account closely related; (*b*) the roots of the podocarps are not "particularly reminiscent" of those of Cordaitales; (*c*) the primitive type of leaf among the araucarian-podocarp alliance was small and narrow and provided with a single midvein and unlike the broad parallel veined leaves of the Cordaitales; (*d*) the structure of the microsporophyll of the podocarps "no more favors this view [cordaitean origin] than the three preceding pieces of evidence." It is scarcely necessary to mention that most of these objections would be equally valid as arguments against a relationship between podocarps and araucarians, a relationship which he champions vigorously, nevertheless. It seems rather unfortunate that so many of the facts known about gymnosperms may be used almost equally well to prove a variety of quite antagonistic views.

Under the direct argument, he places first the similarity of the ovulate cone of the simpler and more primitive podocarps and of the araucarians to the lycopod cone. They are, he thinks, alike in their general structure, being in both cases composed of simple sporophylls with a gradual transition to the leaves. In both each simple sporophyll bears a single median megasporangium. In both the sporangial organ is at first erect in the axil of the scale. In certain of the podocarps he sees a tendency to the development of a double structure of the cone scale analogous to that of the Abietineae.

Secondly, "the microsporophylls are also easily comparable with those of the lycopods." The presence of more than one sporangium in the conifers "is not a serious" difficulty, since septation is well known in the sporangia of lycopods. Moreover, "the shifting of the sporangia to the under side presents little difficulty to the view under consideration," since it "has certainly taken place in other cases." The other cases cited are the *possibility* of its having occurred among the ferns, and the fact that "among the Equisetales in *Palaeostachya* the *sporangiophores* are found on the upper side of the sporophyll, while in *Cingularia* the *sporangia* are below the sporophylls" (italics mine). In any case,

whether conifers have sprung from ferns or lycopods, one sort of sporangium must have migrated to the other side of the sporophyll. This is true, of course, only if one accept the author's view that the cone scales of conifers are really simple.

The presence of seeds in *Lepidocarpon* and *Miadesmia* is held to prove that "there is thus abundant evidence that the potentiality of seed production existed in this phylum as well as in the fern phylum." Any differences that exist in the vascular supply of the sporophylls between podocarps and lycopods "is to be accounted for by the greater relative importance of the ovule as compared with the sporophyll." Small, narrow, uninerved leaves are characteristic of both conifers and lycopods, but are unknown among the Cordaitales. Those podocarps and araucarians with broad parallel veined leaves are not primitive, but have derived their leaves from narrow-leaved ancestral forms.

One of the most interesting points in the argument is the attempt to show that, while a siphonostele with leaf gaps is certainly characteristic of the fern alliances, it is not necessarily limited to them. It merely represents a goal toward which vascular plants of all sorts have tended. The ferns reached it early, while the paleozoic lycopods did not quite reach it. They did actually attain the seed habit, another one of the milestones of plant evolution, but attained only to a medullated siphonostele in which nearly all the metaxylem had been obliterated and which had become broken up in some forms into separate strands. These bundles were still exarch, however, and the leaf traces did not produce leaf gaps in the stele. These would have been the next logical steps in the evolution of the lycopod stele. The inference probably is that they were actually attained by the yet unknown lycopodialean ancestors of the conifers. While admitting that the presence of bordered pits in the secondary wood of conifers is a point against the lycopod theory, he thinks that the presence of a modified sort of pit in *Sigillariopsis Decaisnei* (**48, 53**) shows the possibility of their development in this phylum. The double leaf trace of the Abietineae (and Araucarineae), which has been used as an argument for the cordaitean origin, he thinks is offset by the single trace of the primitive podocarps.

The conclusion is indicated by the following quotations: "To the writer the evidence seems to point . . . . to the primitiveness in the Coniferales of a type bearing female cones composed of aggregations of simple sporophylls, each sporophyll bearing a single erect axillary ovule." "This supposed primitive conifer is very suggestive of the Lycopodiales, but is not reminiscent of the Cordaitales."

It is evident from the preceding that, aside from what support may be gained by discrediting rival theories, the lycopod theory derives its greatest strength from the three following sources.

The first and strongest argument comes from the very close resemblance in form and structure of the ovulate araucarian cone to the strobilus of the lycopods. If there were no other reasons for suspecting a filicinean origin of araucarians, and there were no Abietineae with their perplexing structures, no one would, I think, even suspect that the ovulate cone is other than what it appears to be. Notwithstanding these influences most (though not all, **20, 42, 59**) investigators (**44, 61, 69, 76**) who have studied the araucarian cone have concluded that it is really simple in structure and its cone scales simple sporophylls.

Next in importance and even more difficult to dispose of is the structure of the seed and pollen tube. SEWARD and FORD have pointed out the close resemblance of the seed structure (**54**) and the writer has elsewhere (**7**) shown that these structures could easily have arisen from the condition found in *Miadesmia* and *Lepidocarpon*, but that it is exceedingly difficult to see how, and still more difficult to see why, they should have arisen from the known types of cordaitean seeds. It is easy to see how pollination of the scale instead of the nucellus would be the most probable type in plants which developed the strobiloid habit before the pollination and seed habit. But it seems hardly probable that having been in possession of the habit of depositing pollen grains in a specially prepared and protected pollen chamber in the nucellus, any group of plants would pass through a course of evolution requiring them to give up all the advantages comprised in these arrangements and to acquire an entirely new and certainly less efficient method of pollination. Considering how very little we know of the

structure of paleozoic seeds of any sort, it would be rash indeed to suppose that the known types of seeds were the only ones found among Cordaitales, and even more rash to generalize more than provisionally on the assumption that they did not possess a particular type. We do not know whether the seed habit was developed in the phylum once or more than once, much less whether it was developed before or after the organization of cones. In short, this is today a strong argument for the lycopod theory, that the discoveries of tomorrow may become an equally valid argument for the cordaitean theory.

A third group of resemblances between lycopods and conifers is presented by the leaves. The small, narrow, uninerved type of leaf so characteristic of lycopods is very common among the conifers. The arrangement in many cases is also similar. The gradual transition from leaves to sporophylls in the lycopods presents a very close resemblance to certain araucarians. Most, or perhaps all, of these resemblances, indeed, can be explained away, but that is just where their strength as evidence for this theory lies, they do have to be explained away.

On most other points the theory appears to be on the defensive. It can, to be sure, offer more or less plausible explanations and possibilities for some of the evidence that appears to be against it, but still it must explain them in some other than the obvious way to bring them into harmony with itself. The first and most serious objection in the opinion of most of its opponents appears to be the structure of the stem, more particularly the stele. Notwithstanding STILES's ingenious and convincing exposition of the evolutionary tendencies of the lycopod stele, it yet remains true that no known lycopod did attain to the possession of a mesarch or endarch siphonostele with leaf gaps. That they might have done so appears very probable, but there is yet no evidence that they actually did so, and much less that any one that could be supposed to be a form ancestral to the conifers had even nearly approached it. The same sort of objections apply with even greater force to the attempts to explain the origin of the staminate cone structures. It is admitted by most botanists that septation has probably occurred in certain lycopod sporangia. It may even be

admitted that a shifting of sporangia from one surface of the sporophyll has occurred in some pteridophytes. Again, there is no evidence that they have occurred in any lycopod that can by any possibility serve as a starting-point for modern conifers.

I have indicated above what seem to me to be the most fundamental objections to the theory, namely, that the individual comparisons which can be made between conifers and lycopods must be made with plants of the latter phylum which are admittedly very remotely related to one another. No single lycopod is known that combines within itself any very considerable number of resemblances to the conifers. In a later paragraph I shall return to the attempt to evaluate evidence of this sort. It is an interesting fact that practically all the evidence for this theory is derived from the comparative morphology of adult plants. The two conspicuous exceptions are (1) STILES's comparison of the erect axillary mature megasporangium of the lycopods with the position of the very young ovule of the podocarps, which is also erect and axillary, but which may later be inverted and carried out and away from the axis by the growth of the base of the sporophyll; and (2) his argument that the primitive leaf in the conifers was small, narrow, and uninerved, because many conifers have juvenile foliage of this sort.

The cordaitean theory

The majority of writers have held that conifers are ultimately to be derived from the Cordaitales. SCOTT (**53**), OLIVER (**45**), WORSDELL (**77, 78**), COULTER and CHAMBERLAIN (**16**), JEFFREY (**29, 42**), THOMSON (**70, 73**), and many others have brought forward much convincing evidence in support of this view. Although these authors agree in general as to the ultimate origin of all conifers from a common stock, there is considerable diversity of opinion as to the relationship of the tribes. In the present paper we shall consider these divergent views only so far as they pertain to the origin and relationship of the araucarians. There is a prevailing opinion that this tribe is either the primitive basal group of conifers or constitutes an independent line by itself. Opposed to this view is that of JEFFREY and his students, who have presented much evidence to show that the Abietineae are the oldest and most

primitive group, and that the araucarians have been derived from them.

The first view finds its support in (1) the many close resemblances between the modern araucarians and the paleozoic cordaiteans; (2) the apparently greater geological age of the Araucarineae; and (3) transitional forms of the Triassic and Cretaceous, which appear to become more like the Abietineae from the earliest to the later ones.

The second view does not deny the similarities pointed out as supporting the previous view, but in view of other sorts of evidence believes them to have been secondarily acquired. It does not necessarily deny that they are indications of relationship, but merely that they do not indicate direct and immediate relationship. It derives most of its positive support from (*a*) vestigial structures, (*b*) recapitulation phenomena in seedling and young wood, (*c*) traumatic responses.

Since the supporters of the lycopod theory have chosen the araucarians as the tribe most favorable to their contention, the views of their opponents can be best set forth, perhaps, by marshaling the evidence that tends to show that the araucarians and, by implication, the other conifers have had a direct and immediate origin from Cordaitales.

1. GYMNOSPERMS ARE A MONOPHYLETIC GROUP.—There has been a general tendency toward the view that the gymnosperms resemble one another so much more closely than they do any other group that they must therefore have had a monophyletic origin. This point of view was apparently prominent in the minds of more than one (**45, 52, 77**) of the speakers at the Linnaean Society discussion. If the monophyletic origin of gymnosperms be admitted, it follows almost without dispute that they all have had a filicinean origin. Among the known fossil groups of gymnosperms no other can present anything like so strong a claim to be the ancestors of the conifers as the Cordaitales. Just how numerous, striking, and significant are the resemblances between Cordaitales and Coniferales (more particularly Araucarineae) can be best shown by a brief review.

Ever since JEFFREY'S (**27, 28, 29**) division of vascular plants into Lycopsida and Pteropsida on the basis of the presence or absence of

leaf gaps, there has been a very general disposition to accept this distinction as entirely valid so far as it concerns the Pteropsida. Striking as is this fact in the other groups of the Pteropsida, it is preeminently so among the conifers, a small-leaved group where it is not only present in the mature stem but also in the seedling and reproductive axes (**31**). It is true (**61**) that the cladosiphonic exarch stems of the ancient lycopods did occasionally become medullated, and it is possible that in the course of evolution some member might have lost all of its centripetal wood, and have developed centrifugal wood and leaf gaps, but there is no evidence as yet that any of them ever actually did either.

The histological structure of the stem is only less strikingly uniform among gymnosperms than the general organization. In fact, the wood of araucarians and cordaiteans is so nearly identical that no absolutely trustworthy tests have yet been discovered for distinguishing them. Although the other gymnosperms do not all have exactly the same arrangement of the bordered pits, they do all have such pits on the radial walls of the tracheids, and they are, on the contrary, with a single exception (**48, 53**), unknown among the lycopods. While there is greater diversity in the phloem, perhaps that of lycopods differs still more widely.

Aside from the Araucarineae, the structure of the ovulate cones is more readily brought in line with a filicinean than a lycopodinean ancestry. Though the ovulate cone readily lends itself to the derivation of araucarians from lycopods, it can nevertheless be explained in terms of the Cordaitales; while, on the contrary, it is very difficult to explain the cone of a pine in terms of a lycopod ancestry, and next to impossible to so explain those of cycads.

The structure of the seed is remarkably uniform through the entire phylum, from the oldest to the living representatives. Very few lycopods (**3, 51, 53**) are known to have borne seeds of any kind and even those are much simpler than those of any gymnosperms. I have elsewhere (**7**) pointed out that these seeds do offer us an analogy of the way in which the peculiar pollination processes of the araucarians may have originated. It may be objected, however, and I think rightly, that if lycopods had developed high grade seeds, they would have been likely to parallel the structures

present in the Pteropsida. Still it remains true that they are not known actually to have done so.

There are no microsporangiate structures known among lycopods that are at all comparable with the pollen cones of the gymnosperms. Notwithstanding the puzzling diversity within the group, it is still far easier to derive them from filicinean ancestors than from club mosses. In the latter the sporangia are uniformly single and adaxial instead of multiple and abaxial, as they are in ferns and gymnosperms.

With the exception of the Gnetales, the female gametophyte of the gymnosperms is so uniform in mature structure and in development as almost of itself to preclude any question of its diverse origin. The deep-seated megaspore, the vacuolated free nucleate embryo sac, the centripetal growth of walled tissue, the origin and development of remarkably uniform archegonia are common to all known members of the group, and form a unique and characteristic series unknown outside of it.

The development and mature form of the embryo, with its free nuclear phase, organization into a walled proembryo, elongating suspensors, and terminal embryo, are no less striking and equally without analogy outside the group. Nor is there any sufficient diversity in the mature structure or in the course of development of the male gametophytes to cast serious suspicion on their common origin. The differences are strictly of degree, and find a ready explanation in the changes incident to a long course of evolution.

As a result, it seems to the reviewer that all gymnosperms resemble one another in very many and very significant ways. On the contrary, it is the araucarians alone that present anything more than very slight resemblances to the lycopods, and even here the significant points of resemblance are few and less exact than the numerous ones that relate them to other gymnosperms.

2. THE ARAUCARINEAE RESEMBLE THE CORDAITALES MORE CLOSELY THAN ANY OTHER CONIFERS.—Among those who hold this view no one has expressed himself more clearly or strongly than SCOTT, who says: "The Araucarineae present a close agreement with the Cordaiteae in the structure of the stem, and particularly in that of the wood, which, as universally admitted, is often indis-

tinguishable in the two families. The essential feature is that the mass of the wood, apart from the medullary rays, is composed of tracheids with multiseriate bordered pits on their radial walls" (**53**, p. 654).

The more recent and comprehensive argument for this view is that of THOMSON (**70**). His arguments may be summarized under four heads: (1) the Araucarineae closely resemble the Cordaitales in the anatomy of the stem, root, and leaf; (2) they are the oldest known conifers; (3) certain mesozoic forms show a transition from Araucarineae toward Abietineae; (4) vestigial structures in leaf, stem, root, and reproductive axes, some of which indicate (*a*) the origin of araucarians from cordaiteans, and (*b*) others of which indicate the origin of Abietineae from Araucarineae.

Although I have not seen the papers by GOTHAN (**21, 22**), the references to them by other writers, particularly JEFFREY and THOMSON, would indicate that he holds similar views respecting the relationship between the Araucarineae and Abietineae.

Speaking of the pith, THOMSON (**70**) says: "In the variability of the size of the pith, and in the magnitude which it may attain, the Araucarineae are the only forms of the conifers at all comparable to those of the cordaitean alliance."

The root is usually diarch, and the protoxylem points are separated into two forks by the presence of a resin duct, as in the Pineae (**70**); nor "is there any indication of a resin duct in the center of the metaxylem, as in the Abieteae."

There is a very broad transitional zone from the primary to the secondary wood in the stem and particularly so in the cone axis. "In no other group of the conifers is there an approach to this cordaitean condition" (**70**).

There are many resemblances between araucarian and cordaitean leaves. The araucarian-podocarp alliance includes the only conifers with leaves at all comparable in size to those of the cordaiteans. In both, the leaves persist for many years; in both, the leaves are parallel and dichotomously veined, with mesarch collateral bundles and remarkably persistent leaf traces.

Although the pitting of the more ancient araucarians was so nearly identical with that of cordaiteans, it "is much reduced in the

mature (stem) wood, and occurs mainly at the ends of the tracheids" (70) in the living forms. Some cordaiteans show a notable tendency in the same direction. The characteristic paleozoic type of pitting is found only in the primitive regions of the living forms (cones and roots).

A torus is present in the bordered pits of all conifers except araucarians and some podocarps (70). It is very poorly developed, when present at all, in the Araucarineae, and entirely absent in Cycadales, Ginkgoales, Cordaitales, Cycadofilicales, and Filicales.

Miss GERRY (20) has proposed to separate araucarians from other conifers on the ground that they lack bars of Sanio in the radial walls of the tracheids, which are possessed by all others. JEFFREY has gone even farther and held that it is the most certain distinguishing feature in separating fossil araucarians from the abietineans (35). On the contrary, THOMSON holds "that a rudimentary bar of Sanio is present in all Araucarineae" and "that the araucarians are not to be separated from the other conifers because of the lack of a bar of Sanio, but rather that they are to be regarded as the basic forms from which this structure in the other conifers has been derived" (70). I have found no reference to its presence in cordaitean wood, so that its absence or feeble development is another point of similarity.

The absence of resin canals in the wood of both Araucarineae and Cordaitales is a well known and striking resemblance. THOMSON holds it to be primitive in both cases (70). He brings forward much argument to show that resin canals in the pines are primitively solid, and that they have been derived from resin parenchyma, which has in its turn replaced the resinous tracheids characteristic of cordaiteans and araucarians. He concludes that "the origin of the resin tissue of the pine alliance from tracheary elements as in the Araucarineae, and the retention of similar stages in its development, forms what the writer regards as one of the fundamental features of relationship between the two groups."

So far as living araucarians are concerned, the cells of the medullary rays are characteristically thin-walled and unpitted, just as they are in the Cordaitales. There are known several mesozoic forms in which the rays approach the abietinean type.

It is obvious that forms intermediate in character may be interpreted as araucarians that are being modified in the direction of the abietineans (**70**), or as pines that are about to be transformed into araucarians (**32, 34, 35**). It appears (**70**) that the older forms are more like the araucarians, while the later ones resemble the pines more closely. The geological sequence thus appears to be in favor of the origin of pinelike conifers from araucarian ancestors.

Recently evidence (**75**) has been adduced to show that marginal ray tracheids have arisen through a modification of the tracheids of the wood, and not by a transformation of parenchymatous cells. The oldest known forms with ray tracheids do not antedate the Cretaceous. Since the Araucarineae are known with great certainty from the Jurassic and probably from earlier strata, the geological evidence appears to favor the view that thin-walled unpitted ray cells are the primitive type.

The albuminous cells of the phloem have been considered (**13, 65, 75**) homologous with the ray tracheids. Their absence from the Araucarineae, accordingly, has been interpreted (**70**) in the same way.

Annual rings are absent or feebly developed in the Cordaitales and in most Araucarineae, though STOPES (**64**) has recently reported a cretaceous *Araucarioxylon* from New Zealand with very definite growth rings.

Aside from the structure of the seed itself, the ovulate cone of *Araucaria* is nearer to that of the Cordaitales than are those of the Abietineae. The essential feature of the cordaitean cone is that the single seed is borne on an axis standing in the axil of a bract. The seed-bearing axis is not always axillary if one may trust the illustrations (**16**). In some cases the bract appears to be borne on the seed stalk. The seed itself is terminal and erect. The cone of *Araucaria* differs in that the bract and axis are much more intimately associated and in that the ovule is inverted and not terminal. There are at least three obvious interpretations of the cordaitean cone. First, one may suppose the cone to be simple, consisting of an axis covered with branched sporophylls, some of which are sterile and some fertile. Secondly, one may suppose that the cone is compound and that the ovule is borne directly on the branch

axis, which stands in the axil of a bract or leaf, or merely arises directly from the main axis among the bracts but without a fixed relation to any of them. Thirdly, one may apply the theory of CELAKOVSKI (16) of the pine cone to it, and suppose that the seed-bearing axis really represents a branch, in the axil of a bract, so intimately united with a sporophyll, which itself bears the seed, that no traces are left of its complex nature.

Without attempting to review the extensive and well known literature relating to this third theory, the writer is disposed to admit that it offers a reasonable explanation of the cones of the Abietineae. It seems much less probable when applied to the araucarian cone or to the ovulate structures of the podocarps and taxads. The attempt to explain the cordaitean cone according to it would appear to be beset with very many grave difficulties. In the first place, most of the evidence used to support it for the modern forms is here unavailable. In the second place, there is no indication in the cordaitean cone itself of such a union of branch and sporophyll. In the third place, it is hardly to be supposed that if such a process had taken place in the ancestors of the pines, there would be still in the present geological age clear indications of it, and that the paleozoic ancestors would have apparently gone so much farther than their modern representatives as to have made their cone appear even simpler than any of them, including even the apparently simple araucarians. Evolution plays strange tricks, it is true, but it really puts a considerable strain on one's credulity to believe, as I think we must if we accept the theory that araucarians are derived from Cordaiteans *through the Abietineae*, that a complex branch system was reduced to a cone in the paleozoic cordaiteans, showing practically no trace of its complexity, then reverted in the abietinean descendants to a stage where the evidence of complexity is again clear, and finally passed on into the araucarians, where the evidence of complexity is again at least doubtful.

It seems to the writer far simpler to make no such difficult assumptions, but to consider that the cones of *Cordaites* and *Araucaria* are no more complex than they appear to be. In any

case, the cones of the two resemble one another closely in apparent structure, and will probably both be eventually satisfactorily explained in the same way.

The seed of *Araucaria* resembles that of *Cordaites* in having the nucellus free from the integument to a zone below the female gametophyte. It is doubtful whether this is a character of any great consequence, inasmuch as there were seeds of both types known among paleozoic plants. Nevertheless, it remains true that this ancient type is not known in any other modern plants. Cordaitean seeds, so far as yet definitely known, appear to have had pollen chambers in the nucellus. Since the pollen has been found in these chambers, in some cases apparently sealed in, one can only infer that the method of pollination was essentially the same as that of modern Abietineae. In this respect the Araucarineae differ very markedly. To the writer the difference appears so great, and the method of the Cordaitales so much superior, that it is difficult to believe that having been once attained it would ever have been given up (**36**). If there is any dependence to be placed on the facts that appear to indicate that podocarps have been derived from an araucarian ancestry, it would appear that the tendency of evolutionary selection had been in this case in the other direction. This objection does not appear to be very formidable at present, for we know many more seed genera from impressions than we have plants to assign them to. Moreover, we know the internal structure of very few of them. It is not unreasonable to suppose that the Cordaitales may have borne more than one sort of seed, and that among them may have been some which were pollinated in the araucarian fashion.

When the pollen cones are considered, it is at once evident that the closest resemblances to those of the Cordaitales are found in three rather widely unrelated groups: *Ginkgo*, araucarians, and taxads. Doubtless they have all inherited their resemblances from a common source, though along little related lines. So far as the evidence goes, it constitutes a notable resemblance between the araucarians and cordaiteans, in which the abietineans do not share. The araucarian type, with its free pendent sporangia, has apparently been transformed into the more common conifer type with two

imbedded sporangia in the podocarps. It would appear more probable that this transformation had taken place in several lines of descent than that it had taken place in the supposed *Cordaites-Pinus* line, and had then reversed itself in the supposed *Pinus-Araucaria* line. The only very obvious difference between the microsporophyll of *Araucaria* and *Cordaites* is that the pollen sacs are erect and terminal in the latter and reversed in the former. This is precisely the difference in the ovulate cones.

But little evidence can be gathered from the gametophytes, owing to our ignorance of those of the Cordaitales. In both araucarians and cordaiteans the male gametophyte (**4, 5, 36**) is larger than in other modern conifers. It is uncertain whether the gametophyte of the cordaiteans had a more extensive prothallial tissue, like that of the araucarians (**5, 7**), or a more extensive spermatogenous tissue, like that of certain modern cycads (**36, 42**). If I correctly apprehend the abietinean theory of the descent of araucarians, it involves the assumptions that the original male gametophyte possessed a more or less extensive prothallial tissue and at least one antheridium; that in the course of evolution it lost its prothallial tissue with the exception of two primary cells, but retained the spermatogenous tissue of the antheridium (this would perhaps represent the cordaitean stage); that it further lost all of its spermatogenous tissue during its evolution into the Abietineae, except that part giving rise to two male cells; and, finally, that in the course of the evolution of an abietinean into an araucarian the place of pollen deposition became shifted (for reasons not stated) to a point much farther away from the female gametophyte, thereby necessitating the production of a more extensive prothallial tissue (**36**) to supply the needs of the larger amount of cytoplasm required to fill the more extensive pollen tube. Such a course of evolution is presumably possible, though I am inclined to think that the evidence favoring it is yet very inadequate. In the present state of our knowledge the large size of the gametophytes is a point of resemblance between araucarians and cordaiteans, while the pine type of male gametophyte can be easily derived in the same manner from either by the reduction of either or both the prothallial or spermatogenous tissue.

Not enough is known about female gametophytes to make a comparison of much value. The cordaiteans apparently possessed apical archegonia in the manner of all modern conifers. A comparison of embryos of course is at present impossible.

3. THE ARAUCARINEAE ARE VERY ANCIENT PLANTS.—Although wood of the *Araucarioxylon* type is known from the Paleozoic to the present, it is not yet possible to say with certainty just how old are plants corresponding in other essential points with modern araucarians. SEWARD and FORD (**54**) have given us a very careful review of the fossils that have been assigned to the araucarians, to which SCOTT (**53**) has given general agreement. It appears probable, though not beyond question, that such genera of the Permo-Carboniferous as *Walchia* and *Voltzia* were more nearly allied to araucarians than to any other known conifers (**54**). *Voltzia* and *Ullmannia* appear very probable triassic representatives (**24**). There is abundant evidence of impressions, cones, and wood of araucarians in the Jurassic and Cretaceous (**53, 54**).

The Abietineae have been said to extend to the Paleozoic (**33**), and this assertion has been vigorously disputed. The carboniferous form has been discredited on the ground that its source is not known to be from rocks of that age (**21, 22**). The form from the Permian is said by THOMSON and ALLIN not to be a *Pityoxylon* at all, but a cordaitean or *Araucarioxylon*. PENHALLOW appears to have originally regarded it as a *Pityoxylon* on account of what he supposed were horizontal resin canals (**46**). These are now (**71**) said, on a reexamination, not to be resin canals at all, but leaf traces. If these forms are rejected, no true Abietineae are known that can be compared in age with the araucarians.

4. TRANSITIONAL FOSSIL FORMS.—Of late there have been described (particularly by JEFFREY) a number of mesozoic plants with wood more or less intermediate between the true *Araucarioxylon* and abietinean wood. As will be shown in a subsequent section, the JEFFREY school interprets these as evidence of the origin of araucarians from the Abietineae. THOMSON (**70**), however, points out that the earliest of these transitional forms, *Woodworthia* (**38**), is much more like true *Araucarioxyla* than the later ones (as *Araucariopitys*), while the latter are much more

abietinean. He refers particularly to the absence of resin canals, even of the revival type, in the former, and their presence in the latter. The rays of the latter are thick and pitted in the fashion of the Abietineae, while those of the former are thin and resemble those of modern araucarians as well as cordaiteans. In like manner the pitting of the former is more extensive, more crowded and flattened, and with the pits mostly alternately arranged; whereas in the latter they are less numerous, more restricted to the ends of the tracheids, less crowded, and more frequently opposite. He contends that this is consistent either with a cordaitean or an araucarian ancestry for the pines, but difficult to reconcile with an abietinean ancestry of araucarians.

5. VESTIGIAL STRUCTURES, RECAPITULATION, TRAUMATIC REACTIONS.—The broad transitional zone between primary and secondary wood in the araucarian cone has already been mentioned as a remarkable parallel to the condition found in the cordaitean stem. The pitting in the cone is also more extensive. The pits cover the whole radial surface of the tracheids, are crowded and mutually flattened, and there may sometimes be as many as five rows to a tracheid. THOMSON remarks (**70**) that not only does the pitting in the araucarian cone resemble cordaiteans, but that "instead of the opposite pitting, the pitting of the cone axis and early wood of the Abietineae has characteristically either scattered uniseriate pits or biseriate ones which are alternately arranged."

A torus, characteristically present in mature wood of Abietineae and feebly developed in mature wood of Araucarineae, is entirely absent (**70**) in such primitive regions of the latter as cone axis, first-year stem wood, primary and young secondary root wood. They should be expected in some or all of these places if araucarians had descended from abietinean ancestors which possessed them.

Bars of Sanio are well developed in the Abietineae and feebly so in the araucarians (**42, 70**). They are also poorly developed in the primitive regions of Abietineae and in the mesozoic *Pityoxyla*. From this THOMSON infers that well developed bars of Sanio were not characteristic of the ancestors of the pine alliance.

He also argues that, since resin canals are ontogenetically developed from solid parenchyma and are frequently solid in the abie-

tinean cone and in certain fossil forms, resin canals are not actually a character, as JEFFREY maintains (**42**), of the ancestors of modern Abietineae, and much less so of the ancestors of modern araucarians.

The absence of ray tracheids from the seed cone of *Pinus* and of the erect cells of the phloem from the cone and first few years' growth of the stem and root (**13, 70**) is interpreted to mean that these structures have been acquired in the comparatively recent geological history of the group, very probably long since the time at which Araucarineae are supposed to have originated from it.

In the outer extremities of the vascular bundles of the leaf of the Araucarineae there is a considerable amount of centripetal xylem. It has been interpreted in various ways. THOMSON holds that, though much of it is of the transfusion type, there is yet always a certain number of elongated elements that are true centripetal wood lying next the protoxylem (**70**).

Attention has often been called to the fact that seedling pines have only primary needle leaves and only later develop spur shoots. If they are ancestral to araucarians, the latter might be expected to develop spur shoots on the seedling.

Traumatic reactions play little part in THOMSON'S argument, though he does invoke its aid in the attempt to show that the ancestral type of resin canal (**70**) in the pines was solid. He points out that the resin canals produced by wounding modern pines are much more numerous than can reasonably be expected to have been the case in the ancestral forms, and that they are frequently solid. The argument would appear to cut both ways. In a later paper (**73**) he has studied the normal variability of the spur shoot of *Pinus* and has made free use of the effects of wounding to substantiate his conclusions. His general conclusions are that the larger numbers of leaves on the less definite spurs of fossil forms find their counterparts in the normal variations in the number of leaves to a fascicle of living pines, particularly such wide variations as occur frequently on very vigorous branches, on reproductive axes, and on vigorous seedlings. Variation in the number of needle leaves, branching of the spur shoot, the production of primordial leaves on branches, and even on proliferating spur shoots can all

be produced by wounding. He interprets these reactions as a reversion to the ancestral condition as indicated by their occurrence in the fossil forms and in the primitive regions of living forms just mentioned. LLOYD in a recent paper (**43**) has touched the same subject and given his approval to the general conclusion as stated by THOMSON. The latter calls attention to the fact that the conifers which do not have spur shoots show no evidences, either in their primitive regions or as the result of wounding, that they have descended from ancestors possessing spur shoots.

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THE EFFECTS OF ILLUMINATING GAS ON ROOT SYSTEMS

CONTRIBUTIONS FROM THE HULL BOTANICAL LABORATORY 205

EDWARD M. HARVEY AND R. CATLIN ROSE

(WITH NINE FIGURES)

Introduction

The injurious effects of illuminating gas upon trees and shrubs have been testified to by numerous observers. In 1864 GIRARDIN (5) reported severe injuries to trees in several cities of Germany which he attributed to escaping illuminating gas. Similar observations have since been recorded by many writers, among whom are VIRCHOW (17), KNY (8), SPÄTH and MEYER (14), EULENBERG (4), WEHMER (18), SHONNARD (13), and others.

The shade tree commissions of every city find themselves face to face with this serious problem. The trees of our city streets and parks are unusually subject to the various tree-injuring agencies, the chief of which are insects, fungi, and atmospheric and soil impurities. The two latter factors are particularly characteristic of the troubles of city trees. The problem of gas injuries, therefore, is one of considerable economic importance. City foresters should know the exact cause of any tree death, not only to enable them to provide means for future protection, but also in order to determine with whom responsibility rests for the present financial losses. They should therefore be able to say with certainty whether or not a tree has been killed by gas in the soil. At present this is no small undertaking, because there are few, if any, reliable symptoms known by which one may differentiate with certainty gas injuries from those due to several other causes. For example, fungi sometimes quickly become prominent in a tree injured by gas, as purely a secondary effect (see KNY 8 and STONE 15); but in a case like this the primary injury might easily be attributed to the fungi. It is claimed that characteristic odors often accompany gas poisonings; sometimes in the

roots as well as in the soil; in fact, these odors form the chief diagnostic characters. STONE (15) claims much for them, and it is probably true that in many cases they furnish some evidence. However, many instances are known where illuminating gas odors have not given sufficient evidence for fixing responsibility. A rational system of diagnosis of injuries in city trees would be of value and would be welcomed by all parties concerned. No such system, from the gas injury standpoint, is possible, owing to the lack of definite knowledge concerning the effects of the constituents of illuminating gas under various controlled conditions.

The experimental work previously done on gas injuries to root systems will be briefly summarized. KNY (8) passed known amounts of illuminating gas through the soil at the roots of a maple and two linden trees. Among the symptoms of injuries recorded were the bleaching and final fall of leaves and the appearance of a blue coloration in the xylem of the roots. Finally the trees died. SPÄTH and MAYER (14) passed small amounts of gas into soil in which grew a number of species of trees and shrubs. General death resulted, but otherwise the only symptom recorded was the yellowing of the leaves. BÖHM (1) grew willow slips in water into which he had passed a stream of illuminating gas. The short roots produced soon died, but the slips themselves lived for three months. Potted plants of *Fuchsia* and *Salvia* died after gas had flowed to their roots for four months. Again, he found that when gas had been passed through a soil for a long period of time, this soil became very toxic to seedlings germinated in it. Also a *Dracaena* transplanted to this soil became dry and died in ten days. MOLISCH (10) found that roots of corn increased in diameter and were bent in certain concentrations of illuminating gas. SHONNARD (13) noted exudation of sap from the trunk and branches of a lemon tree treated with gas in the soil. RICHARDS and MACDOUGAL (11) found that carbon monoxide and illuminating gas retarded the rate of elongation of roots of *Vicia Faba*, sunflower, wheat, rice, etc. Swelling also appeared in the leaf sheaths of wheat, being somewhat more pronounced in illuminating gas than in carbon monoxide. Such increases in thickness were largely due to the enlargement of the cortical cells. In some cases, however,

the cambium seemed to have become more active. Recently STONE (**16**) has reported proliferations of tissue at the lenticals of willow slips growing in water which had been charged with illuminating gas. He also noted a rapid proliferation at the cambium in stems of *Populus deltoides* due to the influence of gas.

Another important phase of the gas injury problem is that after trees have been killed by gas, a question arises regarding the safety of resetting trees where the dead ones have been removed, assuming, of course, that the gas leak has been located and stopped. It seems to be the general opinion that resetting should only be done either after a considerable time has elapsed, or after large amounts of the old soil have been removed and replaced by fresh soil. Neither of these methods of procedure is entirely satisfactory; the first involves great loss of time, the second is expensive. The practicability of resetting trees in any given case is often determined only by the crude method of smelling a handful of soil taken from the place of injury, and if the odor of gas is still present, resetting is deemed unsafe. One is thus led to ask whether the odor itself is a true index of the toxicity of the soil to the roots of plants.

The investigation reported below was undertaken with the two problems in mind: (1) that of determining some of the effects of illuminating gas on root systems, having in mind the securing of further diagnostic characters of gas poisoning; and (2) whether the chief causes of injury are those constituents of illuminating gas which are readily absorbed by the water film of the soil particles, or those which remain mainly in the soil interstices (not being so readily soluble).

Methods and materials

The illuminating gas used was the so-called "water gas" from the Chicago Gas Light and Coke Company's system. Along with the illuminating gas experiments, many parallel ones were carried out with an ethylene-air mixture. The Chicago illuminating gas contains 2–6 per cent ethylene; therefore, to facilitate comparison between the ethylene alone and the ethylene of the illuminating gas, the ethylene of the mixture above was made to constitute 4 per cent (by volume). Thus, volume for volume, the ethylene-air

mixture and the illuminating gas contained approximately the same amount of ethylene. In the following experiments, where the term "ethylene in corresponding concentrations to the illuminating gas" is used, it refers to the ethylene content of the latter gas. For example, the ethylene concentration corresponding to an illuminating gas concentration of one part gas to four parts of air ("1:4") would be one part ethylene to 100 of air ("1:100"). Such parallel experiments were considered important owing to the fact that ethylene has been found to be by far the most toxic constituent of illuminating gas for the aerial organs of several plants (see CROCKER and KNIGHT **2**, KNIGHT and CROCKER **7**, and HARVEY **6**).

The experiments were primarily arranged so as to yield evidence with regard to the two phases of the second problem above. The methods will be described separately and in the order named. Investigation of the second problem should yield data with regard to the first.

A. THE TOXICITY OF THE CONSTITUENT OF ILLUMINATING GAS ABSORBED BY THE SOIL

Good potting soil in 10–20-liter cans was treated with illuminating gas by allowing the gas to flow through at a definite rate, at room temperature, for varying lengths of time. The rate of flow was approximately 2 liters per hour. The time periods were from 30 hours to 20 days, hence the lots of soil received 40–1000 liters of gas. The moisture content of the soil was kept as near the "optimum" as possible. In one experiment 10 liters of soil received gas at the rate of about 0.2 liter per hour for 68 days. In another experiment 8 liters of soil received gas at the rate of 2 liters per hour for 53 days, and was kept at a temperature of 1–5° C. throughout the period. The purpose of this soil treatment was to allow the soil particles to absorb as much of the gas constituents as possible. In the case of treatment at low temperature, it was the intention to allow still better opportunity for condensation of substances on the soil particles. After stopping the flow of gas, the soils were removed from the cans and thoroughly stirred in pure air to free them from the gas in the interstices. They were then taken to the greenhouse, placed in shallow boxes, and planted to 41 different species of plants,

representing 18 families. Controls were maintained throughout. The seeds were allowed to germinate and grow for periods of 25–60 days.

B. THE EFFECT OF ILLUMINATING GAS ON ROOTS WITH NO SOIL PRESENT

1. *With roots alone exposed to gas*

In order to expose the roots without exposing the shoots, the following method was employed. Moist-air chambers were made from 8-liter, wide-mouthed bottles; 6–8 short glass tubes were inserted in the corks of each of these bottles so that the tap roots of young seedlings of *Vicia Faba* could be admitted to the chamber. The space between a root and the sides of a tube was sealed by means of a short length of pure gum tubing which had been previously fitted to the outer end of the glass tube. Definite amounts of illuminating gas or ethylene were admitted through a small glass tube reaching to the bottom of the bottle. The pressure resulting from the addition of the small volume of gas was relieved through a second short tube.

2. *With entire plant exposed to gas*

Seeds of tomato, radish, and mustard were allowed to germinate, and when the hypocotyls had reached a length of 0.5 cm. they were transferred to flower pots under bell jars provided with water seals. The seedlings were fixed to the rims outside of the flower pots, so that the roots in growing would hang free in the air. Definite amounts of illuminating gas could be easily added.

C. EFFECT OF ILLUMINATING GAS ON ROOTS GROWING IN SOIL MEDIUM

1. *Quantitative tests*

In these tests young seedlings (two or three months old) of *Catalpa speciosa*, *Ailanthus glandulosa*, and *Gleditschia triacanthos* were used. A few days before the beginning of the experiments, the seedlings were transplanted from the pots, in which they had germinated, to large battery jars filled with coarse quartz sand. Two glass tubes for admitting gas were thrust into the sand and

then the roots and glass tubes were sealed in with a vaseline-paraffin mixture by the BRIGGS and SHANTZ method. The total volume of the interstices (allowing for a definite water content) was previously determined, and upon the basis of this volume, known concentrations of gas could be secured. The duration of experiments was 5–21 days. Gases were changed every three days by drawing pure air through the jars for 10–15 minutes by means of an aspirator, and new gas added. Parallel experiments with ethylene were also run in many cases.

2. *Qualitative tests*

In this series of experiments it was desired to subject a number of plants to illuminating gas under conditions met with in the field where leaking gas mains are involved. Potted woody plants, including 36 individuals of 11 species, were used. Illuminating gas was allowed to pass slowly into the soil around the roots. Meanwhile symptoms of injury were noted. When the plant had died, or become seriously injured, or after a certain time had elapsed, the roots were washed clear of soil, and careful examinations made. The rate of gas flow was often less than one-eighth liter per hour.

Results and discussion

The results will be presented in the order in which the methods were described.

A. EFFECT OF ILLUMINATING GAS ON SEEDS PLANTED IN SOIL PREVIOUSLY TREATED WITH GAS AND THEN AERATED

In all cases, at the time of the planting of the seeds, the treated soil gave an exceedingly strong odor of illuminating gas. Of the 41 species planted, 5 failed to germinate, but the failure was in both the controlled and the treated soils. Throughout the 25–60 day period, the aerial portions of the seedlings were watched for signs of injury. However, it was found that the plants in all the treated soils gave no superficial evidence of injury. All seemed perfectly normal. At the end of the period the plants were taken up, their roots washed free of soil, and examinations made. Only two species gave any evidence of injury; these were cotton and lupine. In them the root systems were perhaps somewhat less developed in the treated soils, and in the cotton there appeared to

be a greater development of anthocyanin in the treated soils. At the close of the experiments, the treated soils always gave a noticeable gas odor and in most cases the odor was very strong.

The results of these experiments would indicate that of the constituents absorbed from illuminating gas by soil, those which give the odor to the gas are very prominent, and when once absorbed are held for extended periods, even after the soil is freely exposed to the air. But the most important fact from the standpoint of the question in hand seems to be that these odorous constituents are evidently not extremely toxic to plant roots growing in the same soil. The plants tried in such soils included a wide range in regard to relationship, and also they were taken at what is considered the critical stage, that is to say, the germinating and young seedling stages. Just what these odorous compounds are is an interesting question. The odor of any illuminating gas is probably the combined odor of a number of substances, for example, pyridine, thiophene, picoline, quinoline, cumene, cymene, and others. Very little is known concerning the effect of these on vegetation. CROCKER, KNIGHT, and ROSE (3) found cumene, thiophene, and pyridine were many hundred times less effective in reducing growth in the etiolated sweet pea seedlings than was ethylene.

The results of the experiments described indicate that the presence of a gas odor in soil is not an index of its toxicity for vegetation, and that odors would be valuable merely as a means of determining whether or not illuminating gas had been in the soil. With regard to using odors in diagnosis, CROCKER has suggested to us the possibility of distilling (at high temperature *in vacuo*) soil from places where gas injuries have been suspected, but where the odor even at the time is not discernible, thereby drawing off some of the odors previously held too firmly by the soil particles.

B. THE EFFECT ON ROOTS WITH NO SOIL MEDIUM

1. *With roots alone exposed*

Material, seedlings of *Vicia Faba*. Exposure period five days.

With illuminating gas.—(1) Concentration 1:4000; no effects were noted, evidently grew as well as controls; (2) concentration 1:400; growth in length somewhat retarded and two other strongly

marked effects entered: (*a*) swelling back of the root tip, and (*b*) turning and coiling of root (fig. 1); (3) concentration 1:40; growth in length greatly retarded, also considerable swelling; coiling present, but the coils smaller and tighter.

With ethylene.—(1) Concentration 1:100,000 (that is, corresponding to ethylene of illuminating gas with (1) above); apparently no effect; normal growth ensued; (2) concentration 1:10,000; responses similar to those shown in fig. 1 but somewhat more pronounced; (3) concentration 1:1000; little or no growth in length; strongly swollen, no coils.

The parallel experiments with ethylene gave some evidence that the toxic effect recorded for the illuminating gas may be due to ethylene present in that gas because corresponding concentrations of illuminating gas and the ethylene-air mixture gave quite parallel results.

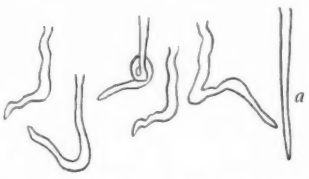


FIG. 1.—Outline of *Vicia Faba* roots, showing the effects of illuminating gas on growth; *A*, control.

2. *With entire plants*

Material, radish, mustard, and tomato. The roots and hypocotyls gave responses as follows:

Radish.—Exposure period 48 hours.

With illuminating gas.—(1) Concentration 1:500; bending of root evident (similar to those of fig. 1); no coiling; no swelling of hypocotyl or root; (2) concentration 1:5000; coiling and bending of root evident; no enlargement of hypocotyl or root.

Mustard.—Exposure period 48 hours.

With illuminating gas.—(1) Concentration 1:100; coiling and bending of root evident (similar to those of fig. 1); swelling of hypocotyl evident; no swelling of root; (2) concentration 1:11,000; bending of root slight; no other effect; (3) concentration 1:20,000; bending of root slight; no other effect.

Tomato.—Exposure period 3 days.

With illuminating gas.—(1) Concentration 1:500; swelling between stem and root (fig. 2); growth considerably retarded; (2) concentration 1:10,000; swelling as above, but less marked;

swollen zone longer and not so thick; more growth of hypocotyl; (3) concentration 1:100,000; little if any effect.

With ethylene.—(1) Concentration 1:12,500; short swollen knob between stem and root; growth greatly retarded; (2) concentration 1:250,000; swollen knob longer and not so thick as in (1); (3) concentration 1:2,500,000; little if any effect.

The response shown by the tomato seedling differs very markedly from that shown by the radish and mustard seedlings. While the roots of the radish and mustard seedlings show a coiling and

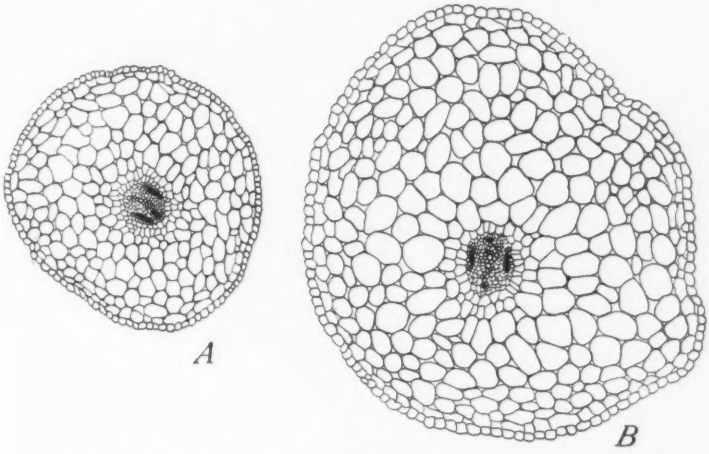


FIG. 2.—Sections of the root of tomato seedlings: *A*, control; *B*, treated with illuminating gas; ×18.

bending similar to those of the *Vicia Faba*, in addition to a slight swelling of the hypocotyl, the only response shown by the tomato seedling is a decided swelling of the hypocotyl and root at the point where the two join. With the tomato seedling the parallel experiments with ethylene again give some evidence that the toxic effect recorded for illuminating gas is due to the ethylene constituent of that gas.

The results recorded above for *Vicia Faba*, radish, and mustard show that injuries to roots may readily be brought about by placing them in an atmosphere containing small amounts of illuminating gas

or ethylene. If we consider here also the results of the first series of experiments, it would seem that the chief causes of injury to root systems are those constituents of illuminating gas which are present mainly in the interstices of the soil, rather than those dissolved in the soil water.

The greatest danger in replanting where other plants have been killed by gas, seems to lie in the constituents remaining between the soil particles. If, therefore, a method was devised for quickly aerating this soil, trees might be safely planted at once without removing large amounts of soil.[1]

C. THE EFFECTS OF ILLUMINATING GAS ON ROOTS IN SOIL MEDIUM

1. *Quantitative tests*

a) *Catalpa speciosa* seedlings; illuminating gas; exposure period 8 days; concentrations 1:2000, 1:400, 1:200, 1:40, 1:20, and controls.

Stems and leaves showed no modifications; neither were there any strongly marked effects on the root systems. In concentrations 1:40 several roots gave indications of swelling 1–2 cm. back of the tips, while in concentrations 1:20 these swellings were very evident.

b) Same as above, with the exception of ethylene in place of illuminating gas but in corresponding concentrations (that is, 1:50,000, 1:5000, 1:1000, and 1:500).

No effect on stems or leaves. The responses with concentrations 1:1000 and 1:500 were like those above, but in addition, the tendency of the roots to coil at the tips (as noted in *Vicia Faba*, fig. 1).

c) *Catalpa* seedlings; illuminating gas; duration of exposure 21 days; concentration 1:4 and controls.

Stem and leaves gave no response. Roots of 1:4 gave swelling of main root extending from near the surface of the sand 4–7 cm. downward (fig. 3). This increase in amount was 2–3 times that of the normal thickness. The epidermis was often cracked and sloughed off in places. The drawing of fig. 4 shows that the swelling

[1] Perhaps this could be accomplished by passing pure air through the soil by means of a pipe thrust below the surface.

is brought about through an increase in size of the cortical cells, and particularly by rapid proliferation at the phellogen layer.

d) *Ailanthus* seedlings; illuminating gas; exposure period 15 days; concentrations 1:400, 1:10, and controls.

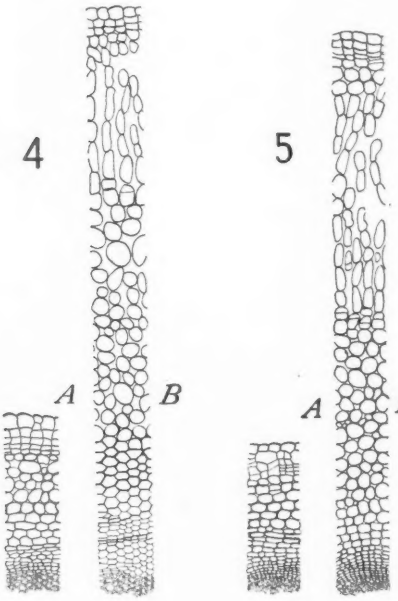
1:400 gave slight swelling in tap root near the surface of the soil; while in the 1:10 the leaves began falling after 5 days, and had all fallen before the end of the exposure. The tap roots were much swollen for 3–4 cm. below the surface of the soil.

e) Same as (*d*) but with ethylene instead of illuminating gas; concentrations 1:10,000 and 1:250.

Swollen zone of tap root was somewhat more pronounced in the 1:10,000 than in the 1:400 illuminating gas above. In the 1:250 all plants had lost their leaves within 8 days, and the tap roots were much swollen. The morphological nature of these swellings may be seen in figs. 5 and 6. In fig. 6 are two outlines of transverse sections of the tap roots of *Ailanthus* seedlings in the region where swellings take place; *A*, the control plant; *B*, a treated plant. Fig. 5 shows the detailed structures of the regions outlined in *A*

FIG. 3.—*Catalpa* seedlings: *A*, controls; *B*, treated with illuminating gas.

and *B* respectively of fig. 6. Through an examination of figs. 5 and 6 it becomes evident that the stelar region has remained unchanged, while the cortex, extending to the phellogen layer, has increased in thickness, partly through increase in the diameter of the cells, and partly through cell division. At the phellogen layer of the treated root, cell division has been rapid, resulting in the production of a loose tissue not present at all in the normal. The tissue lying outside the phellogen layer in the beginning has been only slightly modified in the abnormal root.



FIGS. 4, 5.—Fig. 4., section through the cortex of tap root of *Catalpa: A*, control; *B*, treated with illuminating gas; fig. 5, same for *Ailanthus* roots, showing detail of the tissue regions outlined in fig. 6; ×48.

f) A number of tests were carried out with *Gleditschia* seedlings, used in illuminating gas in various concentrations up to 1:3 (concentrations higher than 1:3–1:4 were not used for fear of oxygen becoming a limiting factor). Concentration 1:3 gave leaf fall, but no definite injuries were detected in the root system.

2. *Qualitative tests with potted woody plants*

When illuminating gas was passed rapidly through the soil of a potted plant, injuries might be observed the first day and death in all cases in the course of a few days. This is not at all surprising, because one should expect injury and final death to result as an effect of the shutting off of the oxygen supply, even though an

inert gas (that is, nitrogen or hydrogen) be passed through the soil in this manner. Accordingly, illuminating gas could do great damage to vegetation independent of any direct toxic property. This view is advanced by KOSAROFF (**9**), who found that wilting took place if a stream of carbon dioxide or of hydrogen was passed through soil in which roots were growing. In all experiments where illuminating gas was passed into the soil rapidly enough to cause the death of the plant, the symptoms manifest in the aerial parts were of a type which would indicate that the injury might be due simply to the cutting off of the water supply as a result of injury to the root system, and not, necessarily, due to a conduction of toxic substances to them. With high evaporating conditions, the symptoms of gas injury (that is, wilting, yellowing, and falling of leaves in the aerial portions) always became manifest very much sooner than under conditions favoring a low water loss. It was found that when roots were killed quickly by using high concentrations of gas, few if any symptoms other than odors appeared which might enable one to diagnose the death as specifically gas poisoning. With regard to the injury to parts above soil, STONE (**16**) believes that "poisonous principles" are absorbed by the roots and conducted upward to the leaves, hence the yellowing and wilting would result as a direct poisoning of them.

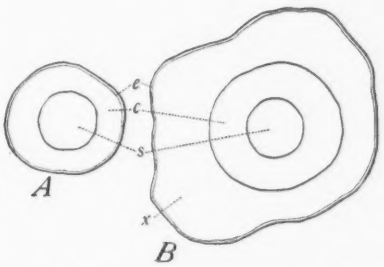


FIG. 6.—Diagrams of transverse sections of *Ailanthus* roots; *A*, control; *B*, treated with illuminating gas; *s*, stele; *c*, cortex; *e*, epidermis and tissue lying outside of phellogen; *x*, new tissue developed by the phellogen; ×6.

When illuminating gas was so regulated that it passed very slowly to the roots (sometimes as little as 40 cc. per day), a stimulation of the roots often took place, resulting in the development of new tissue resembling those reported from the quantitative experiments made with young tree seedlings. Stems were likewise affected when covered by soil or when slightly above the surface.

In stems this proliferation of cells became first evident at the lenticels by their increase in size and the protruding of white tissue, as shown in the lower portion of the stem in fig. 7. RICHTER (**12**) has noted a similar proliferation of tissue at the lenticels of *Vicia villosa* under the influence of tobacco smoke. STONE'S similar results have been cited. In the roots this tissue was abundantly developed in some cases, particularly in *Hibiscus*, which is shown in fig. 7, where *B* and *C* are plants which were subjected to a slow stream of illuminating gas for a period of 30 days. Fig. 8 shows detailed structure of these proliferations in the roots of *Hibiscus*. The cork

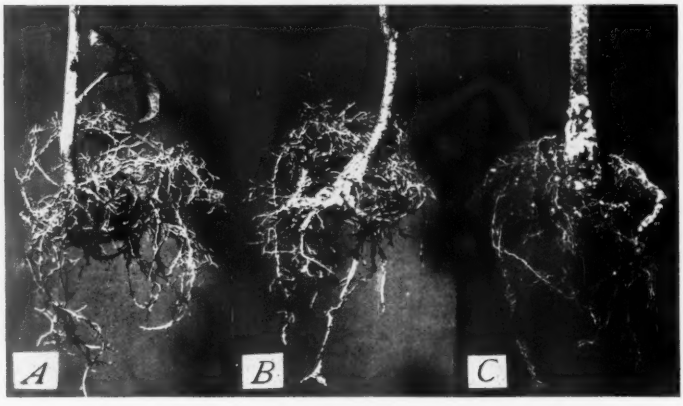


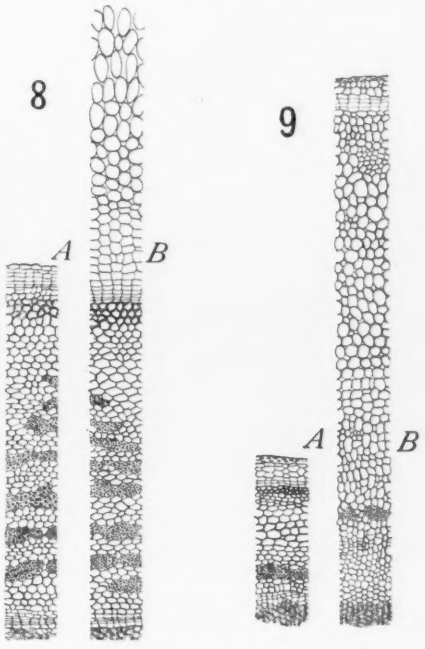
FIG. 7.—*Hibiscus: A*, control; *B*, *C*, treated with illuminating gas

layer of the abnormal root has been sloughed off. Practically the same morphological situation appears in this case as in *Ailanthus* previously figured.

A similar response by lilac is shown by drawings of fig. 9. These abnormalities were recorded for *Hibiscus*, lilac, *Croton*, *Diervilla*, *Ricinus*, *Ulmus*, and pear.

An experiment similar to the foregoing was carried out with an *Ailanthus* growing on the campus near the Hull Botanical Laboratory. The tree had a diameter of about 8 cm. and a height of 3.5 m. At a short distance were other trees of the same species which served as controls. Illuminating gas was admitted to the roots of the

experimental tree through a glass tube thrust 0.7 m. into the soil, 0.6 m. from the base of the tree. The rate of flow of gas was 1.5 liters per hour. The experiment began July 3, and the gas was stopped flowing September 2. Except for two or three short periods, amounting in all to less than three days, the flow of gas was continuous during the two months. Thus the soil near the tree must have received 1.5–2 cubic meters of gas.



FIGS. 8, 9.—Fig. 8, transverse sections through cortex of *Hibiscus* at base of stem; ×40; *A*, control; *B*, treated with illuminating gas; fig. 9, same for lilac; ×35.

The first symptoms of injury were manifest July 14. Leaves of some of the young shoots, growing on the same side of the tree from which the gas entered the soil, showed signs of wilting. Three days later these leaves and others had shriveled and died, but remained attached to the shoots. In some cases only a portion of a leaf was injured. A few leaves of older branches also wilted, but there was no general effect evident throughout the tree at that time. In the middle of September the apparently unaffected leaves began to fall, and finally the tree was free from leaves much before those of the controls. In October the tree looked as though it were entirely dead, and when an examination was made, after it had been removed from the soil, such was found to be the case. A general dryness of the tissues was noted, but neither anatomical changes nor gas odors were detected in them.

During the gas flow, the odor of gas was evident only at the point where the tube entered the soil. In order to determine whether the gas might be detected by a more delicate means, the etiolated sweet pea seedling test, as described by KNIGHT and CROCKER (**7**) was tried. When the seedlings were 1.5–2 cm. high, the petri dishes containing the seedlings were taken to the *Ailanthus* tree, placed directly on the soil, and the 10-liter cans inverted over them. The cans, each with a dish of seedlings beneath, were numbered and placed, with reference to distance from the point of entrance of gas, as follows: no. 1, 5 cm., toward tree; no. 2, 0.6 m., at base of tree; no. 3, 0.8 m., also near tree but on side opposite no. 2; no. 4, 1.2 m., on side directly opposite point of entrance of gas; no. 5, 3 m., as no. 4. The cans were placed August 27 and observations were made three days later. The results were as follows: no. 1 gave no growth (observations refer to epicotyls only); no. 2, some growth, with a little swelling; no. 3, growth slightly reduced and diageotropic; no. 4, slender and straight, 9–10 cm. high (normal); no. 5, as no. 4. These results indicate that the pea seedling probably offers a rather delicate test for the presence of illuminating gas in the soil. In this case the injurious effect on the seedlings was very evident in those placed near the base of the tree, where, as stated before, no odor of gas could be detected in the customary manner.

In consideration of the great difference in behavior of a given plant when exposed to low and to high concentrations of illuminating gas, it seems appropriate to make the following suggestions with regard to the diagnosis of gas injuries. One reason why the foregoing abnormal tissue developments have not been recorded in trees killed by gas is probably the fact that examination was made for pathological symptoms in trees which have died or have become seriously injured. Such conditions would mean that the gas had been at their roots in too high concentration to allow the stimulating effects to enter. Sometimes the proliferation reported above was found in roots when no suggestion of injury could be observed in the aerial portions. Therefore, when one is attempting to diagnose with certainty a serious injury suspected to be due to gas, he ought also to make an examination of roots of other trees in the vicinity which have not yet shown injuries in the leaves, thereby

perhaps adding one more line of evidence. Another change found to take place under the influence of low concentrations of illuminating gas was the disappearance of starch from the cortex of the roots, an observation in agreement with that of Richter (**12**) and others.

Summary

1. When illuminating gas is passed through soil, the odor-giving constituents of the gas are readily absorbed by the soil particles and strongly held.

2. These odorous substances are very slightly, if at all, toxic to roots of plants growing in a soil containing them.

3. The constituents of illuminating gas which remain in a gaseous state in the soil interstices are the chief cause of injury to root systems.

4. Among these constituents, ethylene is probably the most harmful, except in extremely high concentrations of illuminating gas, where the toxicity of other substances, together with other factors, would be expected to play a part.

5. Low concentrations of gas induce abnormal development of tissue.

a) Illuminating gas.—These abnormalities appear in certain tree seedlings within 8–21 days, with concentration one part illuminating gas to four parts air (air of the soil), or as low as one part illuminating gas to forty of air.

b) Ethylene.—This gas alone, when used in concentrations corresponding to the ethylene content of the illuminating gas used in the tests, gives abnormalities similar in type and degree.

6. High concentrations of illuminating gas result in the rapid killing of the roots, and the only symptom of injury to be observed is death.

7. If illuminating gas is allowed to flow very slowly through a soil in which woody plants are growing, abnormal tissue development in the root will very often ensue.

8. In low concentrations of illuminating gas, hydrolysis of starch and some other related chemical reactions are accelerated.

9. It was found that, by use of the etiolated sweet pea seedling, small amounts of illuminating gas in the soil could be detected where the odor of gas was indistinguishable by the usual methods.

We wish to acknowledge our indebtedness to Dr. WILLIAM CROCKER of this laboratory for many valuable suggestions during the course of this investigation.

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NOTES ON NORTH AMERICAN WILLOWS. II[1]

CARLETON R. BALL

(WITH THREE FIGURES)

The first paper[2] in this series dealt with four western willows, including two new species, one new variety, and one new combination. The present paper contains one new species (§ PHYLICIFOLIAE) and descriptions of typical *Salix sessilifolia* Nutt. and of the plant which is usually described in manuals of botany under that name.

The writer wishes to acknowledge here the courteous assistance of Professor C. V. PIPER in communicating his notes on the types of NUTTALL's *Salix sessilifolia* and other species in § LONGIFOLIAE, studied by him at the British Museum. The same acknowledgement is gladly given to Dr. C. F. MILLSPAUGH of the Field Museum and to Dr. B. L. ROBINSON of the Gray Herbarium, and to their associates, for the loan of material representing the species discussed.

Salix pennata, n. sp. (fig. 1).—Low shrub with dark, divaricate, stoutish, glabrous branchlets and large chestnut buds; leaves obovate or elliptic-obovate, 3–6 cm. long, acute, narrowed at base, entire, very dark green above, glaucous beneath, the raised midrib and parallel primary veins conspicuous beneath, glabrous; aments

[1] Since the above was in press the writer has collected true *S. sessilifolia* Nutt. on the Umpqua River at Roseburg, Oregon, and on the Willamette River at Corvallis, Oregon. In both localities it was very abundant, and associated with *S. bolanderiana* Rowlee. It was not found around Portland nor along the Willamette River and adjacent sloughs for a distance of about 15 miles below Portland, including part of the famous Sauvie's Island. The species here treated as *S. fluviatilis* Nutt. is a common plant about Portland, from Ross Island, 2 miles above the city, to Sauvie's Island, some 15 miles below. It is the dominant willow at the Dalles, Oregon, just east of the Cascade Mountains. Both here and on the lower Willamette it apparently is the only species of the *Longifoliae*.

[2] BALL, CARLETON R., Notes on North American willows. I. BOT. GAZ. 40:376–380. *pls. 12, 13.* 1905.

FIG. 1.—*Salix pennata* Ball: a portion of the pistillate type (*Suksdorf* 15 from Mt. Adams) in Bebb Herb., Field Museum of Natural History; nat. size.

sessile, stout; pistillate 2.5–7 cm. long; capsule subsessile, 6–8 mm. long, densely silvery pubescent; style about 1.4 mm. long, stigmas long; scales obovate, acute, black, densely pilose; stamens 2, filaments glabrous, free.

Low shrub, probably about 0.5–1 m. in height, of spreading habit; bark (probably) dark gray to brownish, branchlets stoutish, divaricate or somewhat virgate, dark brown to black, dull or somewhat shining; smooth or sometimes longitudinally finely wrinkled, glabrous throughout; buds mid-sized to large, 6–10 mm. long, ovate, apiculate, chestnut, glabrous.

Leaves usually exstipulate, petiolate; stipules wanting on fruiting branchlets, on one sterile shoot (*Applegate* 2758) 4–8 mm. long, lanceolate to semicordate, acute; petioles slender to stoutish, 5–10 mm. long, greenish yellow or becoming brown, glabrous; blades on fruiting branchlets narrowly to broadly obovate or obovate-oval, acute to abruptly acute at the apex, cuneate to acute at the base, 3–6 cm. long, 1.5–3.5 cm. wide, common sizes being 4 by 2, 5 by 2.5–3, and 6 by 2.5–3.4 cm., those on luxuriant sterile twigs elliptic-oblanceolate to elliptic-oval or even elliptic-oblong, larger, 4–8 cm. long, 1.5–3.8 mm. wide, the narrowest 7 by 1.5, the widest 8 by 3.8 cm., all entire or distal leaves on sterile twigs crenate or crenulate, dark green and somewhat shining above, strongly glaucous beneath, midvein and primary veins rather prominent above, strongly so beneath, where the secondaries make a strong reticulation, the primaries simple, parallel, glabrous throughout or the under surface sparsely sprinkled with short shining hairs.

Aments precocious, stout, sessile, ascending or spreading, loosely to closely arranged on the branchlet, the staminate naked, the pistillate subtended by 1 or 2 very small, scalelike leaves, the peduncle in fruit becoming 2–5 mm. long.

Pistillate aments 2.5–5 cm. long in flower, becoming 3–7 cm. long in fruit, 1.2–2 cm. wide; capsule 6–8 mm. long, narrowly lanceolate-rostrate, acute, subsessile, densely pubescent with silvery white hairs; pedicel stout, 0.2–0.4 mm. long, pubescent; style 1.2–1.5 mm. long, entire, brown; stigmas 0.4–0.6 mm. long, mostly divided; scales in both sexes narrowly obovate, acute or obtusish,

1.2–1.5 mm. long, black, densely clothed on both surfaces with long, straight, shining, white or yellowish hairs.

Staminate aments stout, sessile, 2.5–3 cm. long; stamens 2, filaments slender, 6–8 mm. long, glabrous, free, anthers short, oval; scales as described or somewhat narrower, hairs white or flavescent.

Salix pennata (fig. 1) is easily one of the most beautiful species of § PHYLICIFOLIAE. In relationship it lies between *S. chlorophylla* and *S. pulchra*. Geographically, also, it occupies a position between these two species. It is abundantly distinguished from both. From *S. chlorophylla* it is separated by the much larger and darker leaves, obovate rather than elliptical, with their conspicuous parallel primary veins, especially on the lower surface, and by the longer aments and capsules. From *S. pulchra* it may be distinguished by much the same series of characters. While *S. pulchra* also has large leaves, they are more rhombic-oblanceolate than obovate in outline, always bright and shining green above and less strikingly veined. The stipules in that species also are linear-lanceolate, glandular, and very persistent. Owing to the cold and wet situations in which it occurs, *S. pennata* is a late-flowering species. All the specimens bearing flowers were collected in July, while fruit may be found well into August.

Our species is also an interesting example of apparently restricted distribution. So far, collections have come only from the two great peaks of the central Cascades, Hood and Adams, which face each other across the gorge of the Columbia. Further search may reveal the species on Jefferson and Rainier. The specimen collected by SUKSDORF in Skamania County, Washington, on July 31, 1883, and the battered, fragmentary specimen of the Wilkes Expedition are somewhat doubtfully referred here. Both have leaves more nearly oblanceolate than obovate, but otherwise they have the characters of our species. *S. chlorophylla* and *S. nelsoni* have never been found in the Cascades, though little *S. monica* is found in the central Sierra Nevada.

SPECIMENS EXAMINED.—OREGON: Marion County: 10 miles west of Olay Butte, in alder swamp at head of a canyon, *E. I. Applegate* 2758, Sept. 4, 1898 (N); north base of Olay Butte, in wet meadow, alt. 4000 ft., *Applegate* 2766, Sept. 6, 1898 (N); Clackamas County: edge of swamp, 10 miles north of Olay Butte, alt. 4000 ft., *Applegate* 2770, Sept. 6, 1898 (N); vicinity of Mt. Hood: in swamp, north base of Mt. Hood, *Thomas Howell*, Oct. 2, 1886 (FBb); Government Camp, Mt. Hood, edge of wet meadow, *E. I. Applegate* 2801, Sept. 12, 1898 (N); *F. A. Walpole* 354, Aug. 28, 1899 (N), growth low, dense, compact, 3–4 ft.; shore of Lost Lake, Mt. Hood, *H. D. Langille* 20, July 6, 1901 (B, N).

WASHINGTON: Mt. Adams and vicinity: Mt. Paddo (Adams), *W. N. Suksdorf*, July 31, 1883 (B, N); mountains of Skamania County, *Suksdorf* 1371, Sept. 6, 1883 (B, N); Mt. Paddo (Adams), *Suksdorf* 15 (pistillate type), 16 (staminate type), 17, July 13, Aug. 12, 1886 (FBb); no locality, U.S. Exploring Expedition under Capt. Wilkes, interior of Washington Territory (N).

SALIX SESSILIFOLIA Nuttall, N.A. Sylva 1:68. 1842; Bebb (in part), Willows of California 85. 1879; not of most authors (fig. 2).

FIG. 2.—*Salix sessilifolia* Nutt.: a portion of the pistillate twig of *Hall* 474 in Gray Herb.; nat. size.

S. sessilifolia var. *villosa* Andersson, Monog. Sal. 56. 1867; Anders. in DC. Prod. 16²:214. 1868.

S. macrostachya (in part) as interpreted by ROWLEE, Bull. Torr. Bot. Club 27:250. *pl. 9, fig. 5* (named *S. macrostachya cusickii* in the description of the plate but this variety never published). 1900; not of Andersson.

Shrub 2–3 (or more) m. in height; branchlets slender, the older dark brown, longitudinally striated, the younger yellowish, more or less tomentose, those of the season densely white villous-tomentose with spreading white or gray hairs.

Leaves sessile or subsessile, usually stipulate; stipules ovate to lanceolate, acute, entire or occasionally minutely denticulate; petioles none or 2–4 mm. long on vigorous sterile shoots; blades narrowly to broadly lanceolate or elliptic-lanceolate; the smaller rounded, the larger somewhat narrowed and acute at the base, acute or short-acuminate at the apex, terminating in an extremely sharp spinulose point, 2.5–5 cm. long, 8–15 mm. wide, on vigorous sterile shoots larger, 5–7 cm. long, 15–20 mm. wide, all denticulate to spinulosely denticulate (the spinulose teeth gland-tipped, sometimes 1 mm. in length), green and densely to thinly villous-tomentose with spreading hairs on both sides, especially on the midribs and on very young leaves, not obscuring the green color on fully expanded leaves, only the very youngest silvery white in color.

Aments appearing after the leaves, solitary in the specimens seen, terminal on short leafy branches 1–4 or 5 cm. long and bearing several well developed leaves, or old aments apparently lateral and nearly sessile by the seasonal prolongation of the fruiting branchlet from the bud subtended by the apical leaf.

Pistillate aments dense, 4–6 cm. long, spreading or drooping; rachis densely villous; scales mostly deciduous, broadly elliptic-lanceolate, acute, densely villous-tomentose with gray hairs; capsule lanceolate, acute, 5–6.5 mm. long, villous; style and stigmas together about 1–1.5 mm. long, style apparently 0.5 mm. long (difficult to differentiate style from stigma in old dried material), divided; stigmas about 1 mm. long, divided.

Staminate aments not seen on typical material (on approximately typical specimens they are 3–4 cm. long; scales elliptical, pilose-pubescent; stamens 2, filaments pubescent, free, anthers about 1 mm. long).

The types of NUTTALL's numerous species in § LONGIFOLIAE have recently been studied by Professor C. V. PIPER at the British Museum. As a result of this examination it appears, among other things, that the type of *Salix sessilifolia* is quite different from the plant usually described under that name by American authors. From the illustration (fig. 2) and from the full redescriptions prepared, the identity of *S. sessilifolia* should be established readily. It is seen to be a plant with the leaves on the fruiting branches small, truly lanceolate, and actually sessile! The leaves and the twigs of the season are densely clad with a pilose tomentum of gray hairs which are spreading on the petiole and midrib and more or less appressed on the blade. The capsule also is densely pilose, even in age. True *S. sessilifolia* belongs, therefore, in the group containing *S. argophylla* and *S. macrostachya* (as these are at present understood) rather than with the *S. fluviatilis-melanopsis* aggregation. Just what are the limits of variation in *S. sessilifolia*, as well as the more certain identification of the relatives named above, cannot be settled at this moment. Further study of NUTTALL's types and of collections from the type localities will be necessary to a final decision. The plant commonly identified and described as *S. sessilifolia* is here treated provisionally as *S. fluviatilis* Nuttall.

SPECIMENS EXAMINED.—OREGON: *Elihu Hall* 474, in 1871 (FBb 6271, Gray [fig. 2] distributed as *S. desertorum* Richardson; the Gray Herbarium specimen with "*desertorum*" elided and "*S. sessilifolia* var. *villosa*" written in; the specimen in the Bebb Herbarium identified by BEBB as *S. sessilifolia* var. *villosa;* both specimens attested as *S. macrostachya* Nuttall by ROWLEE); *Cusick* (said by ROWLEE to be no. 1514), said by CUSICK (fide PIPER) to have been collected in Linn County, Oregon, probably on the Willamette River (FBb 994).

CALIFORNIA: In most of the Californian specimens having leaves densely villous-tomentose with somewhat spreading gray hairs, instead of villous with appressed silvery hairs, the leaves are much narrower than in the typical *S. sessilifolia*. Such material is found under the names *S. sessilifolia*, *S. argophylla*, and *S. hindsiana*. Two Californian specimens which rather closely approximate true *S. sessilifolia* in habit and in shape, size, and vesture of the leaves are cited below. Their leaves, however, are not so distinctly lanceolate, nor are they spinulose-denticulate so far as observed, but they do possess small stipules. Marin County, *W. H. Brewer* 2360, in 1863 (FBb 6172) or 1866 (G); Nasismento R., *W. H. Brewer* 544, May 3, 1860–1862 (G, on same sheet with the preceding).

J. G. Jack's plant from Grant's Pass, Oregon, Aug. 23, 1904 (G) may represent the glabrate autumnal aspect of the species. The leaves, though broad, are blunter and quite entire. The spinulose teeth may be deciduous, however. *Cusick's* 4497 from Roseburg, Oregon, 1914 (B) is the same as *Jack's* plant, but with densely pilose-tomentose, stipulate, sharply apiculate leaves which are spinulose-denticulate. His no. 4457 differs only in the more linear leaves; both bear foliage only.

SALIX FLUVIATILIS (?) Nuttall, N.A. Sylva 1:73. 1842 (fig. 3).

S. sessilifolia of various authors, as Sargent, Silva N.A. 9:127. *pl. 475.* 1896; Rowlee, N.A. Willows. I. Longifoliae Bull. Torr. Bot. Club 27:250. *pl. 9. fig. 8.* 1900; Howell, Fl. N.W. U.S. 618. 1903; not of Nuttall, N.A. Sylva 1:68. 1842.

Shrub, 2–6 and probably 8 m. in height; branchlets slender, brown, longitudinally striated (probably from shrinking in drying), glabrous, those of the season yellowish, often pubescent.

Leaves subsessile or the larger distinctly petiolate, stipulate on young shoots; stipules ovate to lanceolate, 2–6 mm. long, sparingly denticulate; petioles becoming 5–8 mm. long on the large leaves subtending branchlets, mostly pubescent; blades very narrowly elliptical or linear-lanceolate or those on fruiting branchlets narrowly oblanceolate, acute or short acuminate at the very sharp or spinulose apex, acute or short acuminate at the base, narrowing to the very short petiole, 5–7 cm. long, 8–14 mm. wide, the subtending leaves much larger, 8–10 cm. long, 15–20 mm. wide, all closely to remotely and more or less spinulosely denticulate, especially near the apex; thinly pubescent, becoming glabrate, the young leaves often silvery silky-villous, finally almost glabrous, green above, paler or occasionally slightly glaucescent beneath.

Aments appearing after the leaves, usually clustered, one terminal and 2–5 younger lateral at the ends of leafy branchlets 5–10 cm. long; pistillate aments 3–7 cm. long, 10–15 mm. wide; ovary silvery villous; capsule lanceolate, 5–7 mm. long, brown, villous to thinly villous to glabrate or wholly glabrous in age; style and stigmas together about 1–1.5 mm. long (difficult to determine how much is style), style divided usually to the base, stigmas also divided to the base, scales lanceolate or elliptical, 2.5–3.5 mm. long, sometimes shallowly erose-dentate at the apex, yellow, 3–5-striate, thinly pubescent when expanding to glabrate or glabrous in age (obovate before anthesis).

Staminate aments clustered as the pistillate, 4–5 cm. long; scales as in the pistillate ament; stamens 2; filaments densely pubescent below and thinly so toward the apex, anthers about 1 mm. long.

The exact identity of this plant is uncertain. The descriptions given by NUTTALL of his seven new species in § LONGIFOLIAE are often meager. Frequently, too, there is very little difference in the series of characters assigned

FIG. 3.—*Salix fluviatilis* (?) Nutt.: portions of two fruiting twigs, collected by Howell in Multnomah County, Oregon, July, 1875 (specimens in Bebb Herb., Field Museum of Natural History; left FBb 6575; right, FBb 6576); nat. size.

to two or more separate species. There is some confusion, also, in the types and cotypes of some species. The plant here provisionally referred to *S.*

fluviatilis Nutt. is the species which has been called *S. sessilifolia* by many authors and is so named in most herbaria. It is the species described as *S. sessilifolia* by ROWLEE in his revision of the LONGIFOLIAE. No specimen of NUTTALL'S collecting is known to exist. The species is quite different from the true *S. sessilifolia*. It is closely related to *S. melanopsis* Nutt., a species described from old Fort Hall, near Pocatello, Idaho. It resembles that species in the broad green leaves (fig. 3), often slightly glaucous beneath, and in the extremely long and glabrate or glabrous scales. It differs from *S. melanopsis*, however, in the much longer, acute capsule, initially pubescent, and in the elongated style and stigmas. The style and stigmas, indeed, are very similar to those of true *S. sessilifolia*. It agrees with the brief description of *S. fluviatilis* in the spinulose-serrate, finally glabrous leaves. The capsules, however, are not normally glabrous but sometimes become entirely glabrous in age. The stigmas, also, apparently are not sessile, as NUTTALL records for *S. fluviatilis*. The present species seems to be confined to the lower part of the Willamette Valley and adjacent Columbia River. The type locality for *S. fluviatilis* is "the immediate border of the Oregon (Columbia), a little below its confluence with the Wahlamet." The writer plans to visit the type locality during the present season.

SPECIMENS EXAMINED.—OREGON: Multnomah County: On river banks near water; *Joseph Howell* (145?), July 1875 (FBb 6575, 6576); July 1876 (F 206534); "Oregon, *Howell*" (FBb 4858); *T. J. Howell*, July 1877, distributed by *G. C. Woolson*, no. 362 (G); the first three collections cited above are probably all one and the same thing but do not bear the same full label; Corbett, *F. A. Walpole* 1032, April 29, 1900 (N); Columbia River bottoms: *Thomas J. Howell*, July 1880, as "*Salix sessilifolia*" Nutt. (B, F 206860; FBb 4859; N); Sandbars, Columbia River, *Thomas J. Howell*, July 1880 (as *S. sessilifolia* var. *villosa* Nutt., F 206859; FBb 4860; N, 19244); Portland, between Portland and the mouth of the river, *E. P. Sheldon* 10862, July 10, 1902 (N, 2 sheets); Portland, *Sheldon* 12029, July 9, 1903 (F 216993).

U.S. DEPARTMENT OF AGRICULTURE
WASHINGTON, D.C.

OXIDATION IN HEALTHY AND DISEASED APPLE BARK

DEAN H. ROSE

During an investigation of oxidase activity in the bark of trees affected with Illinois canker, caused by *Nummularia discreta* (Schw.) Tul., there appeared an interesting correlation between oxidation, as measured by BUNZEL'S simplified apparatus (1), and the acidity of the bark extracts. These were made, in a manner to be described later, from material of three different kinds: (1) green bark from limbs of trees unaffected by canker; (2) green and still seemingly healthy bark from trees badly affected by canker; (3) brown diseased bark from around the edges of cankered areas.

Bark obtained during the winter months did not separate easily from the wood, but care was taken to make the removal as complete as possible without scraping or cutting off any of the wood. While the diseased bark may have contained fungus parasites other than *Nummularia*, the precautions taken make this unlikely. All of the really dead dry bark on the surface of the canker was cut and scraped away, until the moist brown or blackened bark around the edge was reached. A strip of this 3–4 cm. wide, including sometimes a little of the green bark next to it, was then cut off down to the wood and used as "diseased bark." The various samples obtained were dried to constant weight at 65–70° C., ground up almost to a dust with a meat-grinder, and stored in glass-stoppered museum jars or tightly corked flasks. The writer realizes, of course, the need of tests on extracts from undried bark, and expects to carry through a series of them as soon as time and other work will permit.

Extracts for the various experiments were prepared under conditions which made them quantitatively comparable. In all cases distilled water was added at the rate of 8.5 cc. per gram of dried ground bark, and toluene at the rate of 0.5 cc. per 100 cc. of water used. The beaker containing the mixture was set on top of an incubator at a temperature of about 29° C. for one hour, during which time the mixture was thoroughly stirred five or six times.

The extract was then filtered through four thicknesses of cheesecloth, and finally through S. & S. filter paper no. 588.

As previously stated, all tests were conducted with BUNZEL's simplified oxidase apparatus. In this there is no provision made for absorption of the carbon dioxide produced, hence only comparative results can be obtained. It is probable, too, as BUNZEL points out (**2**, p. 30), that with no separate alkali solution in the apparatus a longer time is required for the reaction to come to an end, because of the slower absorption of the carbon dioxide by the mixture in the apparatus. This may partly account for the time required in the experiments described later, but it seems unlikely that a mercury rise which continued for 21 days in the absence of a means for absorbing carbon dioxide would have ceased within a few hours with such a means present. So far as BUNZEL's (**2**) published data are concerned, there is no evidence that oxidation would not have continued longer if his experiments had covered a longer period. REED (**3**) and APPLEMAN (**4**), using BUNZEL's larger apparatus, though they do not state whether or not they used the alkali basket, set the limit for completion of the reaction at 2–4 days.

Corrections for temperature variations were made by running with each experiment a blank apparatus containing only water, and subtracting from the readings in the others the reading above zero (negative pressure) in the blank. No readings were taken when the mercury in the blank stood below zero (positive pressure). It was feared that discrepancies might be introduced by the temperature variations, but as a matter of fact the differences between the results in any two comparable experiments were found to be no larger than those between duplicate apparatus in the same experiment.

As a preliminary experiment (no. 1) an extract was prepared, as already described, from dried ground bark (sample 17) of a healthy Ben Davis limb 3 inches in diameter, and the apparatus set up as follows: 8, 9, 10, 11, 12, with 1 cc. ext.+1 cc. H_2O+4 cc. 1 per cent pyrogallol; 13, with 6 cc. of distilled water.

The shaking was performed by tipping back and forth six or eight times the wire culture-tube holder in which the oxidase appara-

tus were fastened. Oxidation, as indicated by rise of mercury (negative pressure really), was not complete even at the end of seven days, as was well demonstrated by later experiments.

TABLE I

MANOMETER READINGS IN EXPERIMENT NO. 1; EXTRACT OF HEALTHY BARK, SAMPLE 17

DAY OF TEST	TIME OF READING	ELAPSED TIME IN HOURS AND MINUTES	TEMPERATURE AT TIME OF READING	MANOMETER READINGS CORRECTED AGAINST AN APPARATUS CONTAINING ONLY WATER					
				Extract unchanged					H_2O
				8	9	11	12	10	13
1st...	3:55 P.M.	0*	36.0	0.00	0.00	0.00	0.00	0.00	0.0
2d...	9:30 A.M.	17:35	35.5	−0.15	−0.20	−0.25	−0.20	−0.20	0.0
	2:30 P.M.	22:35	36.0	−0.20	−0.25	−0.35	−0.30	−0.20	0.0
3d...	5:00 P.M.	49:05	35.0	−0.60	−0.60	−0.66	−0.60	−0.60	0.0
4th...	2:37 P.M.	70:42	33.9	−0.80	−0.75	−0.93	−0.82	−0.90	0.0
	5:00 P.M.	97:05	34.0	−0.80	−0.78	−0.92	−0.95	−0.98	0.0
5th...	8:50 A.M.	112:55	32.0	−0.95	−0.90	−1.08	−1.04	−1.08	0.0
6th...	11:25 A.M.	139:30	34.0	−1.25	−1.25	−1.25	−1.15	−1.30	0.0
7th...	9:00 A.M.	161:05	33.0	−1.40	−1.35	−1.35	−1.30	−1.40	0.0

* Put in incubator at 2:55 P.M.; closed and shaken at 3:55 P.M. to mix thoroughly the extract and pyrogallol; shaken again next morning.

A similar experiment (no. 2) was set up, using the extract of diseased bark from another Ben Davis tree in the same orchard. The results are given in table II.

TABLE II

MANOMETER READINGS OBTAINED FROM EXPERIMENT NO. 2; EXTRACT OF DISEASED BARK, SAMPLE 15

DAY OF TEST	TIME OF READING	ELAPSED TIME IN HOURS AND MINUTES	TEMPERATURE AT TIME OF READING	MANOMETER READINGS CORRECTED AGAINST A TUBE CONTAINING ONLY WATER					
				Extract unchanged					H_2O
				8	9	10	11	12	13
1st ..	4:10 P.M.	0*	36.3	0.00	0.00	0.00	0.00	0.00	0.0
2d...	4:12 P.M.	24:02	35.4	−0.99	−0.95	−1.09	−0.85	−0.85	0.0
	4:42 P.M.	24:32	35.6	−1.00	−0.93	−1.10	−0.95	−0.95	0.0
3d...	2:22 P.M.	46:12	36.4	−1.70	−1.45	−1.70	−1.52	−1.50	0.0
	5:09 P.M.	48:59	37.0	−1.75	−1.50	−1.77	−1.55	−1.55	0.0
4th...	4:45 P.M.	72:35	36.0	−1.75	−1.65	−1.75	−1.80	−1.75	0.0
5th...	8:18 A.M.	88:08	36.2	−1.93	−1.75	−1.93	−1.95	−1.90	0.0

* Put in incubator at 3:00 P.M.; all apparatus closed and shaken at 4:10 P.M.; shaken again next morning.

It is seen from table II that oxidation by the extract of diseased bark is more rapid than by the extract of healthy bark, being greater at the end of five days than that caused by healthy extract at the end of seven days.

These two extracts were titrated immediately after extraction with n/20 NaOH, using phenolphthalein as an indicator. The difficulty of determining the end point, due to darkening of the extract on addition of alkali, was avoided by using the test-plate method described by Miss SCHLEY (5). In this two phenolphthalein solutions are used, one slightly alkaline, the other slightly acid; the end point is reached when a drop of the solution being titrated just fails to decolorize a drop of the alkaline solution and just shows a faint pink tinge in a drop of the acid solution. By this method the following results were obtained, using 20 cc. of extract in 50 cc. of distilled water: to neutralize 1 cc. of extract of healthy bark to phenolphthalein requires 0.86 cc. n/20 NaOH; to neutralize 1 cc. of extract of diseased bark to phenolphthalein requires 0.38 cc. n/20 NaOH.

These titrations, of course, measure only the base-absorbing power of the extracts, not the H^+ concentration, for the extracts are probably mixtures of strong and weak acids and acid salts, all having different degrees of ionization. But since the concentration of free H^+ ions is known to have a marked influence on many of the reactions taking place in living matter (HÖBER, p. 176), it was important to determine just what this concentration is in the bark extracts. In the absence of facilities for doing this accurately by the gas-chain method, tests were made of the effect of diluted and undiluted extracts on the color changes of various indicators. The most clear-cut results were obtained with mauvein, which is known to give the following color changes: yellow at $H^+ = 2 \times 10^{-3}$; green at 10^{-3}; green-blue at 10^{-4}; blue at 10^{-5}; violet at 10^{-6}. In repeated tests on extracts from different samples of diseased bark, mauvein was turned to a definite green, indicating that here H^+ is about 10^{-3}. Extract of healthy bark turned mauvein yellow, indicating that $H^+ = 2 \times 10^{-3}$. It was found, however, that any given quantity of this extract had to be diluted to 2.4 times its original volume to give the same green as the extract of diseased bark; consequently

(not allowing for increased ionization with dilution), H^+ concentration here would figure, not 2.0 times, but 2.4 times 10^{-3}, or $10^{-2.62}$; that is, both indicator and titration figures show diseased bark to be less acid than healthy bark. With this in mind, a reference to tables I and II above shows that oxidation in the BUNZEL apparatus is in inverse ratio to the acidity of the solution being tested.

This correlation between oxidation and acidity seems also to have obtained in REED's work (**3,** pp. 56 and 76), though he fails to bring out the point, where juice of bitter rot apples caused greater oxidation than the juice of more acid healthy apples. The results obtained by BUNZEL (**2,** p. 26) in testing the effect of different concentrations of pyrogallol show that as the concentration decreased from 16 per cent to 1 per cent pyrogallol the negative pressure increased from 2.11 cm. of mercury to 2.86 cm. This may not be due to decrease in acidity, as determined by the pyrogallol concentration (attention is not called to it in BUNZEL'S discussion of the table), but such an explanation seems the simplest and most likely.

Titration and indicator tests on a 1 per cent solution of pyrogallol and on a two-thirds of 1 per cent solution, the strength used in the apparatus if the diluent were only water, gave the results shown in table III.

TABLE III

RESULTS OF TITRATION AND INDICATOR TESTS WITH PYROGALLOL SOLUTIONS

PYROGALLOL SOLUTION	NO. OF CC. OF N/20 NaOH NECESSARY TO NEUTRALIZE 1 CC. OF SOLUTION TO PHENOLPHTHALEIN		H^+ CONCENTRATION AS INDICATED BY METHYL ORANGE	
	1 per cent	⅔ of 1 per cent	1 per cent	⅔ of 1 per cent
Fresh	0.13	0.086	$<10^{-3.33}$	$<10^{-3.33}$
6 months old	0.34	0.23	$3.50\times10^{-3.33}=10^{-2.79}$	$2.33\times10^{-3.33}=10^{-2.96}$

The indicator figures for H^+ concentration of a fresh two-thirds of 1 per cent pyrogallol solution (about 1/20 molecular) agree well with those calculated from the electrical conductivity of such a solution. This is known to be 3.12 μ, whence the percentage dissociation $=3.12\div355$ (μ at infinite dilution) $=0.88$ per cent. If

all the dissociation in pyrogallol at 1/20 molecular concentration is due to H^+ dissociation and only one H dissociates, the H^+ concentration is less than 0.000616 equivalent. If all three H's dissociate it is less than 0.000616×3, or, in other words, the theoretical H^+ concentration of the pyrogallol solution in the apparatus is between 10^{-2} and 10^{-3} or less than either. Because of BUNZEL's statement (**2**, p. 28) that either fresh or old pyrogallol solution may be used it is worthy of note that a 1 per cent solution six months old required dilution to 3.5 and a two-thirds of 1 per cent solution to 2.33 times its original volume before it failed to give the red color with methyl orange, thus indicating for the two-thirds solution an H^+ concentration of $2.33 \times 10^{-3.33}$ or $10^{-2.96}$ (not allowing for increased dissociation with dilution). That is, by slow spontaneous oxidation the H^+ concentration of a two-thirds of 1 per cent solution of pyrogallol solution is increased in six months from less than $10^{-3.33}$ to $10^{-2.96}$. It follows that fresh pyrogallol solution should be made up for each experiment. In this connection it seems rather more than a coincidence that in BUNZEL's experiment 6 (**2**, table VI, p. 28) fresh 1.0 per cent and 0.1 per cent pyrogallol solutions gave, in a test only one hour long, respectively 0.1 and 0.05 cm. higher negative pressure (greater oxidation) than similar solutions one year old.

The reaction to phenolphthalein of the mixtures in the apparatus may be calculated from the data given in table III. For those containing extract of healthy bark, sample 17, it would be $\frac{0.0+0.86+(4\times 0.34)}{6}=0.37$; for those containing extract of diseased bark, sample 15, $\frac{0.0+0.38+(4\times 0.34)}{6}=0.29$. The acidity of the once distilled water was found to be negligible. On account of the correspondence already observed between titration and indicator figures for both the pyrogallol solution and the extracts, the reaction of the mixtures in the apparatus may be taken to represent roughly the H^+ concentration also until further work can be done.

To test the effect of varying the acidity of the extract on the rate and total amount of oxidation, experiment 3 was set up, using

extract of healthy Ben Davis bark, sample 17. The apparatus contained the following solutions: 8, 9, 10, with 1 cc. extract (acidity reduced one-fourth)+1 cc. H_2O+4 cc. 1 per cent pyrogallol; 13, with 6 cc. distilled water; 11, 12, with 1 cc. extract (acidity reduced one-half)+1 cc. H_2O+4 cc. 1 per cent pyrogallol. The results are given in table IV.

TABLE IV

MANOMETER READINGS IN EXPERIMENT NO. 3; EXTRACT OF HEALTHY BARK, SAMPLE 17

DAY OF TEST	TIME OF READING	ELAPSED TIME IN HOURS AND MINUTES	TEMPERATURE AT TIME OF READING	MANOMETER READINGS CORRECTED AGAINST AN APPARATUS CONTAINING ONLY WATER					
				Three-fourths acid			One-half acid		H_2O
				8	9	10	11	12	13
1st ..	4:40 P.M.	0*	33.0	0.00	0.00	0.00	0.00	0.00	0.0
2d...	9:20 A.M.	16:40	33.0	−1.16	−1.14	−0.89	−1.34	−1.32	0.0
	10:00 A.M.	17:20	33.0	−1.19	−1.17	−1.04	−1.55	−1.49	0.0
	4:55 P.M.	24:15	33.5	−1.31	−1.19	−1.14	−1.69	−1.74	0.0
4th...	3:50 P.M.	71:10	34.0	−2.05	−1.82	−1.96	−2.59	−2.64	0.0
5th...	1:40 P.M.	93:00	32.0	−2.39	−2.27	−2.39	−2.92	−2.92	0.0
6th...	4:35 P.M.	119:55	32.0	−2.51	−2.39	−2.59	−3.33	−3.33	0.0
7th...	4:20 P.M.	143:40	33.8	−2.71	−2.69	−2.84	−3.44	−3.42	0.0
8th...	5:50 P.M.	169:10	34.5	−2.94	−2.84	−2.99	−3.49	−3.59	0.0
9th...	8:12 A.M.	183:32	33.1	−2.94	−2.84	−3.09	−3.74	−3.79	0.0

*Put in incubator at 3:10 P.M.; apparatus closed and shaken at 4:40 P.M.; shaken again the next morning.

Experiment 4 was arranged with extract of diseased Ben Davis bark, sample 15, as follows: 12, with 1 cc. extract unchanged (acidity 0.38)+1 cc. H_2O+4 cc. 1 per cent pyrogallol; 11, with 6 cc. distilled water; 8, 9, with 1 cc. extract (acidity 0.29)+1 cc. H_2O+4 cc. 1 per cent pyrogallol. It was thought desirable to run the reactions thus close together (see table V) to test the effect of very small differences in acidity on the rate and total amount of oxidation. The results are given in table V, which summarizes the results from all the experiments described above and from others duplicating them in various ways but not described in detail in this paper.

A study of table V shows that when results are averaged from several different experiments the statement made earlier still holds, that unchanged extract of diseased bark causes greater and more

rapid oxidation of pyrogallol than unchanged extract of healthy bark. Moreover, when the acidity of healthy bark extract is reduced the oxidation is increased, but greatly out of proportion to the reduction in acidity.

TABLE V

SUMMARY OF EXPERIMENTS WITH EXTRACTS OF HEALTHY AND DISEASED APPLE BARK, SHOWING EFFECT OF VARIOUS DEGREES OF ACIDITY OF EXTRACT UPON OXIDATION OF PYROGALLOL

DAY OF TEST	Extract healthy bark, sample 17				Extract diseased bark, sample 15			Extract healthy bark, no. 11	Extract diseased bark, no. 22a
	1	2	3	4	5	6	7	8	9
	*(3.66)	(0.86)	(0.65)	(0.43)	(0.44)	(0.38)	(0.32)	(0.73)	(0.34)
	† 0.83	0.37	0.33	0.29	0.30	0.29	0.28	0.35	0.28
1st.	0.00	−0.26	−1.24	−1.72	−0.87	−0.98	−0.95	−0.26	−0.57
2d.	0.00	−0.61	.....	.....	−1.35	−1.60	−1.45	−0.53	−0.94
3d.	0.00	−0.84	−2.04	−2.68	−1.53	−1.73	−1.76	−0.77	−1.20
4th.	−0.05	−1.01	−2.34	−2.92	−1.63	−1.89	−1.85	−0.86	−1.36
5th.	−0.10	−1.25	−2.49	−3.29	−1.88	−1.95	−2.00	−1.07	−1.50
6th.	−0.13	−1.38	−2.77	−3.43	.....	.....	.....	.....	.....
7th.	−0.17	.....	−2.90	−3.59	−1.89	−2.13	−2.19	.....	.....
8th.	.....	.....	.....	.....	.....	.....	.....	.....	.....
9th.	.....	.....	.....	.....	.....	.....	.....	−1.51	−1.94
	1 test	Average of 6	Average of 3	Average of 3	Average of 2	Average of 6	Average of 2	Average of 2	Average of 2

* The figures in parenthesis indicate reaction of extract.

† The figures in this horizontal line indicate reaction of mixture in oxidase apparatus.

Further proof of the sensitiveness of the oxidase to changes in acidity is found in columns 5 and 6 (table V), where a difference of 0.01 cc. of n/20 NaOH gave throughout the experiment a consistently greater oxidation by the less acid mixture. The figures in column 7 seem unreliable and suggest an error in determination of the acidity. Column 1 shows the great reduction in oxidation produced by increasing the acidity of the mixture in the oxidase apparatus to practically that of the unchanged extract. The figures in columns 8 and 9 are from an experiment in which extracts of healthy and diseased bark were tested at the same time; they show again the correlation between acidity and oxidase activity. This experiment was continued for 19 days, although during the last 4 all oxidation, measured by changes in mercury level, had ceased.

On the nineteenth day the negative pressure was released, all the apparatus closed again, and the test continued for three days more. The results are shown in table VI.

TABLE VI

		Healthy	Diseased
Before releasing.....	19th day of test	−2.03	−2.46
After releasing.....	19th " " "	0.00	0.00
" "	20th " " "	−0.12	−0.08
" "	21st " " "	−0.15	−0.11
		Average of 2	Average of 2

Table VI indicates that oxygen supply is a limiting factor as well as acidity. Its importance should be investigated further. On the twenty-first day, when the experiment was finally closed, it was found that the mixture from the two apparatuses containing healthy bark extract and from the other two containing diseased bark extract had increased from an original acidity of 0.38 in the first case, and 0.28 in the second case, to 0.50 in both (acidity expressed here as before in terms of cc. of n/20 NaOH necessary to neutralize to phenolphthalein). Similar results were obtained in two other experiments in which the reaction had come to an end.

It thus appears that the gradual slowing down of the rate of oxidation in these experiments and in others conducted with similar apparatus is due to increasing acidity and not to chemical combination of the oxidase with some substance in solution, as suggested by BUNZEL (**2**, p. 39). If this be true, the evidence is strong that oxidases are true catalytic agents, prevented usually from bringing about indefinite catalysis by the presence or absence of something which acts as an inhibitor. It is probable that just as enzyme hydrolysis of carbohydrates and other substances reaches a condition of equilibrium because of accumulation of the products of hydrolysis, in exactly the same way, in the BUNZEL apparatus at least, oxidation ceases because of inhibition by accumulated oxidation products. These will include carbon dioxide and acetic and oxalic acids if the decomposition of pyrogallol by oxidase is like that induced by alkalies and the salts of heavy metals. In the presence of O_2 alkalies cause pyrogallol to turn brown with the

formation of carbon dioxide and acetic acid; salts of mercury, silver, and gold oxidize pyrogallol to acetic and oxalic acids. It might appear that oxidation in the larger BUNZEL apparatus, when the alkali basket is used, is greater because of the removal of carbon dioxide, which would tend to increase the acidity if allowed to remain. This can hardly be true, however, since carbonic acid has a low dissociation constant and at the temperature of these experiments would be present in the solutions in comparatively low concentration. Its effect on acidity is but slight at best, and the removal of it merely shortens the time required for the reaction to come to an end. Acetic and oxalic acids are both more important, for two reasons: all of the acid formed remains in solution, and if it is acetic acid, dissociation (and the consequent inhibitory effect) is about ten times as great as for carbonic acid.

An H^+ concentration of 10^{-3} or 10^{-4} is just in the midst of H^+ concentration optima for various enzymes, as given by HÖBER (**6,** p. 721), varying from $10^{-1.5}$ for pepsin to $10^{-8.5}$ for esterase of the blood. No data are given for oxidase, but the work here reported on indicates that the optimum for them is much less than 10^{-3}. If the pyrogallol as used in the apparatus is just about the H^+ optimum, it is easily seen how the plant juice could make the H^+ concentration too high. Then when base is added, the strongest, most highly dissociated acids are neutralized first, and the H^+ concentration drops faster than represented by the degree of neutralization.

It may be possible to get some idea of the H^+ optimum for oxidase activity, and of the H^+ concentration of various mixtures in the apparatus by further work with indicators, but definite knowledge on these points must come finally from careful determinations by the gas-chain method.

Summary

1. Extract of apple tree bark affected with Illinois canker causes greater and more rapid oxidation of pyrogallol than does the extract of healthy bark.

2. Diseased bark extract is less acid than healthy bark extract, according to both indicator and titration figures, hence the

conclusion seems justified that oxidation is in approximately inverse ratio to the acidity of the extract in the range of concentrations here used.

3. This conclusion is borne out by the fact that addition of acid to the solution in the apparatus decreases oxidation and addition of alkali increases it.

4. Oxidases are very sensitive to small variations in acidity of the solution in the oxidase apparatus.

5. As a hypothesis in need of further proof the following is offered. The gradual slowing down of oxidation in the BUNZEL apparatus is brought about by accumulation of oxidation products, probably acetic and oxalic acids, and not by a using up of the oxidase through chemical combination between oxidase and oxidizable substance.

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SANIO'S LAWS FOR THE VARIATION IN SIZE OF CONIFEROUS TRACHEIDS

IRVING W. BAILEY AND H. B. SHEPARD

In 1872 SANIO[1] published the results of an investigation upon the tracheids of Scotch pine, *Pinus sylvestris* L. From his observations and measurements he deduced five general laws which may be translated as follows:

1. In the stem and branches the tracheids everywhere increase in size from within outward, throughout a number of annual rings, until they have attained a definite size, which then remains constant for the following annual rings.

2. The constant final size changes in the stem in such a manner that it constantly increases from below upward, reaches its maximum at a definite height, and then diminishes toward the summit.

3. The final size of the tracheids in the branches is less than in the stem, but is dependent on the latter, inasmuch as those branches which arise from the stem at a level where the tracheids are larger themselves have larger tracheids than those which arise at a level where the constant size is less.

4. In the gnarled branches of the summit the constant size in the outer rings increases toward the apex, and then falls again, but here irregularities occur which may be absent in regularly grown branches.

5. In the root the width of the elements first increases, then falls, and next rises to a constant figure. An increase in length also takes place, but could not be exactly determined.

SANIO's measurements and conclusions have been accepted by DEBARY, PFEFFER, HABERLANDT, and others and have been considered to be applicable to conifers in general.

[1] SANIO, KARL, Über die Grösse der Holzzellen bei der gemeinen der Kiefer (*Pinus silvestris*). Jahrb. Wiss. Bot. **8**:401–420. 1872.

The writers recently had occasion to test the validity of SANIO's first two laws in connection with an investigation undertaken in the endeavor to secure a simple method for segregating long and short "fibers" in the manufacture of raw wood pulp. The following North American conifers were studied: *Pinus Strobus* L., *P. palustris* Mill., *Picea rubens* Sarg., *Tsuga canadensis* Carr., and *Abies concolor* Lindl. and Gord. Chips of wood were removed from the specimens and were macerated by the use of a 5 per cent solution of equal parts of chromic and nitric acid. The tracheids were separated after maceration by being shaken with water and glass beads, insuring minimum breakage. They were then kept in a mixture of 95 per cent alcohol and chloroform to prevent softening. The measurements were made with a micrometer eyepiece, 50 measurements from each chip. The results obtained are shown in the following tables:

EFFECT OF AGE ON THE LENGTH OF TRACHEIDS

TABLE I

Pinus Strobus, 120 ANNUAL RINGS, CROSS-SECTION 1 FOOT FROM GROUND

ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS			ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS		
	Max.	Min.	Av.		Max.	Min.	Av.
20.....	3.35	1.60	2.65	80.....	5.10	2.65	3.77
30.....	3.65	2.35	3.02	90.....	4.60	2.50	3.79
40.....	4.80	2.55	3.64	100.....	4.65	2.50	3.85
50.....	4.20	2.35	3.30	110.....	4.45	3.20	3.91
60.....	4.65	2.45	3.47	120.....	5.40	3.45	4.20
70.....	4.80	2.65	3.52				

TABLE II

Abies concolor, 80 ANNUAL RINGS, ELEVATION OF CROSS-SECTION UNKNOWN

ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS			ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS		
	Max.	Min.	Av.		Max.	Min.	Av.
10.....	3.50	2.20	2.75	50.....	5.45	3.30	4.45
20.....	3.90	2.40	3.30	60.....	5.45	3.55	4.64
30.....	4.90	2.50	3.65	70.....	6.30	3.50	5.02
40.....	5.65	2.15	3.58	80.....	6.45	3.55	5.20

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20.....	3.90	2.40	3.30	60.....	5.45	3.55	4.64
30.....	4.90	2.50	3.65	70.....	6.30	3.50	5.02
40.....	5.65	2.15	3.58	80.....	6.45	3.55	5.20

TABLE III

Pinus palustris, 230 ANNUAL RINGS, CROSS-SECTION 1 FOOT FROM GROUND

ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS			ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS		
	Max.	Min.	Av.		Max.	Min.	Av.
10	3.00	1.80	2.42	130	5.90	3.90	4.99
20	3.85	2.40	3.06	140	6.25	3.75	4.92
30	4.50	2.80	3.70	150	6.00	4.00	4.87
40	4.95	2.80	4.06	160	6.20	4.00	5.08
50	5.30	2.95	4.11	170	6.05	3.45	4.57
60	6.05	3.05	4.33	180	5.75	3.40	4.10
70	5.25	2.85	3.92	190	5.50	3.10	4.03
80	5.70	3.05	4.41	200	6.20	3.15	4.42
90	6.05	3.20	4.66	210	6.05	3.35	4.66
100	6.30	3.45	4.88	220	5.90	3.10	4.35
110	6.05	3.60	4.57	230	5.20	2.85	4.00
120	5.80	3.85	4.66				

TABLE IV

Tsuga canadensis, 80 ANNUAL RINGS, CROSS-SECTION 1 FOOT FROM GROUND

ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS			ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS		
	Max.	Min.	Av.		Max.	Min.	Av.
10	2.30	0.95	1.63	50	4.25	1.85	3.17
20	3.20	1.15	2.20	60	3.80	1.60	2.48
30	3.75	2.05	2.99	70	3.20	1.40	2.47
40	4.00	2.25	3.16	80	3.15	1.80	2.84

EFFECT OF POSITION IN THE VERTICAL AXIS ON THE LENGTH OF TRACHEIDS

TABLE V

Picea rubens, 50 ANNUAL RINGS, AVERAGE TRACHEID LENGTH IN MILLIMETERS

Annual ring	Distance from ground in feet					
	1	6	12	18	24	30
10	1.68	.....	.....	.....	.....	.....
20	2.21	2.49	2.22	.....	.....	.....
30	2.58	2.92	2.84	2.49	1.89	.....
40	3.17	3.00	3.48	3.31	3.23	2.71
50	3.48	3.56	3.84	3.70	3.87	3.31

EFFECT OF WIDTH OF RING UPON THE LENGTH OF TRACHEIDS

TABLE VI

Pinus Strobus, ECCENTRICITY DUE TO FASTER GROWTH ON ONE SIDE, AVERAGE

ANNUAL RING	TRACHEID LENGTHS—MILLIMETERS	
	Narrow	Wide
10	1.63	1.66
22	2.58	2.41
27	2.51	2.53
Average	2.24	2.20

Discussion of measurements and conclusions

The results of these measurements are obviously not in accord with SANIO's first law, since no constant tracheid length was found in any of the specimens examined. Inasmuch as SANIO found a constant in all cases within the 50th ring, and in one case within the 20th, and this study found none, even within the 230th, it is evident that SANIO's first law cannot be applied to conifers, and some doubt is cast upon the accuracy of his observations upon Scotch pine. As is shown diagrammatically in the accompanying figure, the length of the tracheids increases rapidly for a period of years varying from 25 to 60. At the end of this period there is a marked falling off in the length of the tracheids, which lasts for a decade or more. Subsequently the tracheids again increase in length. In the case of the long-leaf pine, which unfortunately was the only very old material available, the tracheid length reaches a maximum at 160 years and decreases, with one marked period of recovery, during succeeding rings. The factor or factors which produce the fairly regular cycles or series of crests and depressions which occur in the long-leaf pine curve are obscure and deserve to be studied carefully by some one who has easy access to old coniferous trees. DOUGLASS'[2] and HUNTINGTON'S[3] interesting correlations

[2] DOUGLASS, A. E., Weather cycles in the growth of big trees. Monthly Weather Review. June 1909.

[3] HUNTINGTON, ELLSWORTH, The climatic factor. Carnegie Inst. Washington, D.C. 1914.

between rainfall and ring width suggest the possibility that the dimensions of tracheids may be equally sensitive to modifying climatic factors.

It is evident from Table V that SANIO's second law is applicable to *Picea rubens* as well as to *Pinus sylvestris*. However, a fact unnoted by SANIO is that the maximum average tracheid length occurs higher from the ground in rings nearer to the bark. This probably bears a relation to the fact that each successive increment is larger, that is, extends farther from the ground. The study of

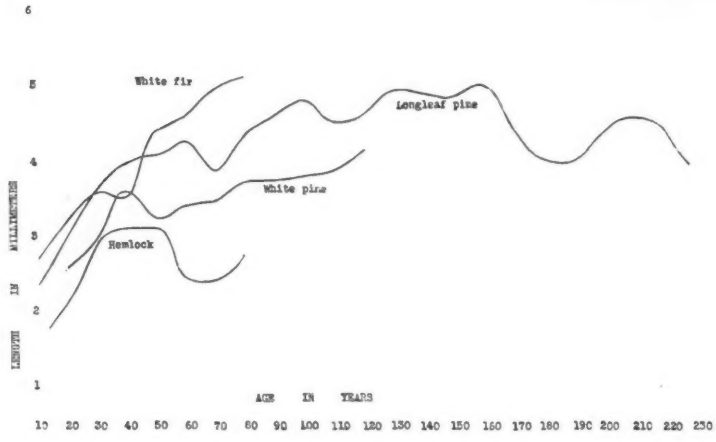

FIG. 1

the eccentric cross-section recorded in table VI seems to indicate that width of ring has no marked effect upon the length of tracheids.

A number of botanists have endeavored to make use of the dimensions of the xylem elements in the classification and identification of the secondary wood of living and fossil plants. When based upon the study of a limited amount of material taken from a given region in a tree, the measurements are significant only if compared with those secured from a homologous region in a tree which has grown under similar conditions. Average dimensions

for a species must obviously be based upon a large amount of material selected from mature plants grown in different environments. However, general averages of this character are of little value for the purposes of the systematic botanist, or in the identification of woody tissues, and are only of relative value to the manufacturer of wood pulp.

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BRIEFER ARTICLES

CHARLES EDWIN BESSEY

(WITH PORTRAIT)

In the history of American botany the name of CHARLES E. BESSEY will always hold a conspicuous place. When his first textbook (*Botany for High Schools and Colleges*) appeared in 1880, it introduced a new era in botanical instruction in this country. Before that date, the study of botany in the United States was bounded practically by the taxonomy of the higher plants, with such gross morphology as enabled the student to use a manual. BESSEY'S *Botany* brought the atmosphere of SACH'S *Lehrbuch* to American colleges, and this compelled the development of botanical laboratories. This original textbook was the first of a series of texts that continued to be very influential.

Professor BESSEY was a most stimulating teacher, and perhaps no American botanist has left his mark upon so many students. Especially in government service and in agricultural colleges are these students to be found, for the teacher believed that his science should be directed toward the public service. At the same time, he was uncompromising in his views as to the difference between botany as a science and its various practical applications.

He was born in Milton, Ohio, May 21, 1845; received his Bachelor's degree at Michigan Agricultural College in 1869; and subsequently studied at Harvard University. He held only two academic positions as professor of botany: the first at Iowa State College (1870–1884), and the second at the University of Nebraska (1884–1915), where he

served also as dean and at various times as acting chancellor. He died at Lincoln, Nebraska, February 25, 1915.

He was a member of many scientific societies, and was very active in promoting their interests. His colleagues recognized his great services by electing him to various important offices. Among these offices were vice-president of Section F of the American Association, a position which he filled four times, (1893, 1894, 1902, 1907); president of the Botanical Society of America (1896); and president of the American Association (1910).

Aside from his preparation of textbooks, his special interests as an investigator were the fungi, the flora of the Great Plains, plant migration, and the relationships of plant families. His later publications dealt with "plant phyla," the last upon this subject appearing in January 1914. These studies of the relationships involved a laborious assembling and comparison of material that few would care to undertake, and the conclusions as to classification were far from stereotyped. For example, instead of the conventional four great groups of plants, Professor BESSEY presented the plant kingdom in fourteen "phyla."

In addition to textbooks and papers upon the special subjects referred to, Professor BESSEY was a regular reviewer of botanical literature for *Science*, so that he was in continual contact with those interested in his subject. His reviews were characteristic of the man, for they were always kindly. Apparently he searched for the pleasant things to say, and left the unpleasant things unsaid.

The botanical fraternity of this country will miss the presence of Professor BESSEY keenly, for his frequent attendance upon scientific meetings made him well known personally to all botanists, and his very genial nature made friends of all his colleagues.—J. M. C.

CURRENT LITERATURE

BOOK REVIEWS

Ascent of sap

DIXON'S[1] volume on transpiration and ascent of sap clearly puts the evidence in favor of the cohesion theory, along with the weak points of the other theories. The chapter headings give an idea of the organization and contents of the work: (1) nature of transpiration and the ascent of sap; (2) ascent of sap in stems; criticism of physical theories; (3) ascent of sap in stems; criticism of vital theories; (4) cohesion theory of ascent of sap in stems; (5) tensile strength of the sap of trees; (6) estimate of the tension required to raise the sap; (7) osmotic pressure of leaf cells; (8) the thermo-electric method of cryoscopy; (9) method of extracting sap for cryoscopic observations; (10) osmotic pressure in plants; (11) energy available for raising the sap.

The cohesion theory of the rise of sap assumes that the water is drawn up through the stem by a pull applied at the top of the water column, and that this pull is transmitted downward through the cohering water. The pull, at least in high transpiration, is due to the evaporating power of the air, but it is limited in amount, as experiments indicate, by the osmotic pressure of leaf cells. The theory excludes living cells along the tracheary system of the stem and root from an active part in the rise of sap. At times of low transpiration, DIXON believes the living cells of the leaf blade may generate the pull by secreting water into the intercellular spaces. For the support of this theory three things require quantitative study: cohesion of sap under conditions existing in the tracheae; osmotic pressure of leaf cells; and total pull required to maintain the water column and to satisfy the transpiration loss in the highest trees at maximum transpiration. It is scarcely less important to know whether there is at all times a continuous water column in the stem of sufficient cross-section to account for the rise by cohesion, and whether actual tensions of considerable magnitude exist in this column. DIXON has apparently answered the first three points adequately, and has brought some evidence for the fourth; while RENNER has shown the existence of tensions of 10–20 atmospheres in the water of the stem tracheae of transpiring plants.

DIXON'S new measurements of the tensile strength of the sap of trees as exhibited in capillary tubes are of great interest. The values range from 45 to 207 atmospheres. The latter, according to DIXON, is the highest experimental

[1] DIXON, H. H., Transpiration and ascent of sap in plants. 8vo. viii+216. *figs.* 30. London and New York: Macmillan. 1914.

value ever obtained for the cohesion of a liquid. The measurements were made with sap containing considerable air in solution, thus approximating the conditions in the tracheae. It is estimated that in the highest trees at maximum transpiration, the cohesion necessary to raise the sap does not exceed 20 atmospheres, a value well within the tensile strength of sap. For determining the osmotic pressure of leaf cells, DIXON has used the freezing point method. To this end, he and ATKINS have improved the current methods of extracting plant saps and have developed a thermo-electric method of determining the lowering of the freezing point. The essential new feature in the extraction of sap is the emersion of the tissue for some time in liquid air. This destroys all living semipermeable membranes and allows the solutes to be pressed out unrestrictedly with the water. The main advantage claimed for the thermo-electric method is the small amount of fluid needed, 2 cc. as against the 15–20 cc. for the ordinary Beckman apparatus. This is frequently a very important consideration for the plant physiologist. With the use of multiple juncture couples, a lowering of the freezing point by 0.0001° C. can be measured as against 0.001° C. for the Beckman. With the single juncture couples, used in the main by DIXON, a lowering of 0.01° C. can be measured. By reducing the size of the Beckman apparatus, BURIAN and DRUCKER were able to measure the lowering of the freezing point of 1.5–2 cc. of solution accurately to 0.005° C., and DRUCKER and SCHREINER by a different method were able to measure the freezing point of 0.005 cc. of a solution accurately to 0.01° C.[2] For the needs of the plant physiologist, I am inclined to think that the latter methods are more suitable.

This book is rather more a statement of the extensive and in the main extremely important work of the author on the rise of sap than a critical consideration of the whole literature of the subject. This accounts for chap. vii, which presents DIXON'S cumbersome and inaccurate method of determining osmotic pressure of cells of the leaf by balancing it against gas pressure; for the slight consideration given RENNER'S work, which proves a critical point for the cohesion theory, namely the actual existence under certain conditions of a stretch of 10–20 atmospheres in the water of the tracheae; for the failure to point out that by a very different method RENNER confirmed a law established by DIXON, namely the rate of flow of a water column through a stem is proportional to the push or pull applied to it; and for the absence of the significant data of HANNIG, FITTING, and others on the osmotic pressure of leaf cells.

As yet the existence of a continuous gas-free water column from root to leaf under all conditions has not been established. Until this is established, there will be a just argument against the cohesion theory of the rise of sap.—WM. CROCKER.

[2] HÖBER, Physikalischechemie d. Zelle, etc., p. 22. 1914; Biol. Centralbl. **33**:99. 1913.

MINOR NOTICES

Plant breeding.—A new edition of BAILEY's *Plant breeding* has been prepared by GILBERT.[3] The preceding edition, the fourth, appeared in 1906. The new edition includes much of the material of the older ones, and in addition presents the results of the more recent investigations in genetics that have removed plant breeding from the simple operations of a few years ago, and have made of it a subject of rigid experimental work, with a wealth of special terminology. Professor BAILEY voices a feeling among the older botanists when he says: "The literature has now become complex and difficult, with considerable gain no doubt in a closer acquaintance with the subject, and a nearer approach to the ultimate truth; but the charm of the simple literature is largely buried, and I fear that much of our interest is now expressed in the discussion of methods and in disputing about the reasons. Yet we are accumulating knowledge, and after a time we shall come back to clarity and to a simplicity that the layman can use."

The titles of the chapters indicate the contents: The fact and philosophy of variation; The causes of individual differences; The choice and fixation of variations; The measurement of variation; Mutations; The philosophy of the crossing of plants; Heredity; How domestic varieties originate; Pollination; and The forward movement in plant breeding.

A most useful feature of the book is the very complete bibliography, which includes 59 pages of citations, extending from 1905 to 1912, inclusive. There are also 27 laboratory exercises, which will be most helpful to teachers who wish to make plant breeding a laboratory course rather than a series of lectures.—J. M. C.

The coco-nut.—Under this title COPELAND[4] has published a detailed account of one of the most valuable plants of the tropics. Naturally the book is of chief interest to those engaged in the culture of the coco-nut palm. In the introduction the origin and spread of this palm as a crop plant is considered. Assent is given to the belief that it is a native of the American tropics, and that in very early times it was carried across the Pacific and introduced into Polynesia, where it has become a crop of the first importance. From Polynesia it reached New Guinea and Malaya, but not Australia. Much more recently it has been introduced along all tropical coasts.

The chapters deal with the physiology of the plant, climate and soil, diseases and pests, selection and treatment of seed, field culture, and products. The products of the coco-nut are considered under four heads: toddy, a general name referring to the sap, which is the source of drinks of various kinds and

[3] BAILEY, L. H., Plant breeding. New edition revised by ARTHUR W. GILBERT. 8vo. pp. xviii+474. *figs. 113.* New York: Macmillan. 1915. $2.00.

[4] COPELAND, E. B., The coco-nut. 8vo. pp. xiv+212. *pls. 23.* London: Macmillan. 1914. 10*s*.

sugar; coir, a commercial name for the fiber obtained from the husk of the coco-nut; copra, the desiccated kernel or oil-cake; and oil.—J. M. C.

Cocoa.—The important cultural plants of the tropics are gradually becoming known from the modern standpoint, and the most recent contribution in this field is the large volume on cocoa by VAN HALL,[5] director of the institute for plant diseases and cultures at Buitenzorg, Java. In the brief historical account it is stated that the first knowledge and use of the plant came to Europe in connection with the conquest of Mexico, where Cortez found it in extensive cultivation under the name "cacao," as he reported it. Some conception of the kind of scientific data included in such a study of an important cultural plant may be obtained from some of the titles of the chapters, such as Geographical distribution and climatic conditions, Chemistry of cocoa and cocoa soils, Botanical characteristics of the cocoa plant, Varieties of cocoa, Cultivation of cocoa, Fermentation, Diseases and enemies, Cocoa-growing countries.—J. M. C.

Flora of California.—Part 5 of *A flora of California* by JEPSON[6] has just appeared. The first parts were noticed in this journal,[7] attention being called to the general high character of the work. This part completes the Portulacaceae, includes the Caryophyllaceae, and begins the Ranunculaceae. A new genus of Caryophyllaceae is proposed (*Eremolithia*), based upon *Achyronychia Rixfordii* Brandegee.—J. M. C.

NOTES FOR STUDENTS

The gases in the floats of marine algae.—This subject has attracted the attention of a number of investigators during recent years. The fact that it is being investigated from so many points of view gives hope that we may soon have fairly complete knowledge of the subject. LUCAS[8] has investigated the subject from the point of view of the origin of the gas. The following Australian marine algae have vesicles filled with gas: Sargassaceae (*Sargassum, Carpophyllum, Turbinaria, Cystophora, Cystosira, Scaberia, Phyllospora*), Fucaceae (*Hormosira*), and Laminariaceae (*Macrocystis* and possibly *Adenocystis*). In young plants, and even in mature fruiting individuals in sheltered situations in shallow water, there are no vesicles.

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[5] VAN HALL, Dr. C. J. J., Cocoa. 8vo. pp. xvi+515. *figs. 140.* London: Macmillan. 1914. 14*s*.

[6] JEPSON, W. L., A flora of California. Royal 8vo. Part V, pp. 465–528. *figs. 14.* San Francisco: H. S. Crocker Co. 1914.

[7] BOT. GAZ. **49**:153. 1910.

[8] Lucas, A. H. S., The gases present in the floats of certain marine algae. Proc. Linn. Soc. New South Wales **36**:626–631. 1912.

There seem to be three possible sources of the gas in these vesicles: (1) atmospheric air, (2) the gases dissolved in the sea water, and (3) gases produced in the metabolism of the plants themselves. He excludes (3) because he has "never detected any gases beyond nitrogen and oxygen in the floats," and "it is hardly conceivable that any process of metabolism should yield these two gases only without any carbon compounds." He excludes (1) because his analyses (table I) do not show the nitrogen and oxygen in the vesicles to be in the proportion that they are in atmospheric air (79:21), and because the gas must be formed in the cavity *pari passu* with its growth, otherwise the vesicles would collapse.

TABLE I

ANALYSIS OF GASES IN THE FLOATS OF ALGAE

Species	Percentage of nitrogen	Percentage of oxygen	Comments
Phyllospora comosa (1).....	86.0	14.0	Fresh-looking plants cast up on the shore
" " (2).....	89.4	10.6	Just cast up
" " (3).....	88.9	11.1	" " "
" " (4).....	82.3	17.7	" " "
" " (5).....	83.2	16.8	" " "
Hormosira Banksii (1).....	88.46	11.54	Growing
" " (2).....	88.0	12.0	Same gathering kept a day longer
Cystophora monilifera.....	80.0	20.0	Floating in the sea

TABLE II

ANALYSIS OF GAS OBTAINED BY BOILING IT OFF FROM WAVE-WATER FROM THE OCEAN

	(*a*)	(*b*)
Nitrogen.....	67.12	58.33
Oxygen.....	26.03	30.55
Carbon dioxide.....	6.85	11.11

He notes that in the analyses of surface waters collected by the "Challenger" the proportion of nitrogen to oxygen varied around a mean of 2:1, and that this would be expected from the relative solubility of the two gases.

He accepts (2), although his table does not show the proportion of nitrogen and oxygen to be the proportion that he has found in sea water gas (table II), and although he finds considerable carbon dioxide in sea water gas and none in the gas from vesicles. He observes that the presence of organisms seems to alter the natural ratio of the gases dissolved from the air. He believes that the gas enters, dissolved in the water, by osmosis, and that just as marine algae show a marked preference for potassium sulphate and commonly reject much of the sodium chloride, so they may show a marked preference for oxygen, and

the useless nitrogen together with the oxygen not required may be eliminated and set free in the floats "in order to serve a mechanical purpose." He suggests that since the proportion of oxygen in the vesicles is less than in atmospheric air or in air dissolved in sea water, the plant must use up some of the oxygen "for other purposes than levitation." This last suggestion evidently means that the gas is modified by the metabolism of the tissues through which it passes.

In regard to (3) we shall await with interest to see whether the absence of carbon dioxide from the floats of marine algae is confirmed by other workers on other species.

On the whole, the paper is a clear, clean-cut statement of the problem and of the results of the investigation, although in a few places its scientific value might be improved by assuming a less teleological point of view.—GEORGE B. RIGG.

Cecidiology.—Among the American papers by botanists is one by STEWART[9] on the gall caused by *Andricus punctatus* Bassett. The author has made a very careful study of the histology of the gall, which he has compared with the histology of tissues formed in the healing of wounds. In his summarization on this point he says:

> The following conditions in this gall can be correlated with similar conditions in traumatic tissue: (1) a recapitulation of similar conditions of ray structure; (2) a vertical shortening of the broad rays; (3) the presence of ball-formations in the wood, which appear only in tangential sections; (4) a parting of the fibers in the vicinity of the larval chambers, similar to the condition resulting from longitudinal wounds; (5) isodiametric parenchyma cells around the base of the larval chambers with irregularly distributed fibers and other woody elements among them; (6) a great reduction in the number, or an entire lack of vessels; (7) a shortening of many of the cells of the wood; (8) absence of distinct annual rings of growth; (9) a suggestion of the return of the cambium to normal activity after a time; (10) woody inclusions in the bark.

Unfortunately, the bibliography is not satisfactory in that it includes some papers which have much less bearing on the subject than certain papers which are not included. Also, some of the statements in the first part of the paper concerning our knowledge of the anatomy of galls are misleading. The reviewer also questions the statement of the title of the paper, in that the inclusion of the generic name of the gall insect would have made the title much more suggestive to those who may wish to become informed on the literature of the subject. However, the author has brought out a number of points which are frequently overlooked by students of abnormal plant growths.

Another very excellent paper is by LUTMAN[10] on club root, in which the author discusses the relation of *Plasmodiophora Brassicae* to its hosts and the

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[9] STEWART, ALBAN, Notes on the anatomy of the *punctatus* gall. Amer. Jour. Bot. 1:531–546. *pls.* 2. 1914.

[10] LUTMAN, B. F., Studies on club root. Univ. Vermont, Bull. 175. pp. 27. 1913.

structure and growth of its plasmodium. The parasite gains entrance to the host through the epidermis or the root hairs, and thence from cell to cell either by penetrating the cell walls or by division of the host cells, each daughter cell containing a part of the parasite. The parasite is most abundant in the cortex, but is always found in the central cylinder. The infected cells hypertrophy, but there is no serious interference in nuclear or cell divisions until the disease is well advanced, when both processes are suspended. Although the author has not made a definite statement on this point, it appears from the general discussion and the drawings that the hypertrophy is most pronounced in the primary cortex. This is to be expected, but a careful study to determine the extent to which the various groups of cells are susceptible to enlargement as a result of stimulation by the parasite would be well worth while. The discussion of the character and life history of the parasite is excellent.

A brief paper by HEALD[11] gives a description and results of inoculation experiments with a gall on *Prosopis glandulosa*. The gall is probably due to *Bacterium tumefaciens* Smith and Townsend.

Among the most important contributions to cecidology by American entomologists, we find a brief paper by COCKERELL[12] on a mite gall on the red orpine (*Clementsia rhodantha* Gray), one of the Crassulaceae growing in the high altitudes of the Rocky Mountains. The flowers "were aborted to a dense, round mass of a dark crimson-like color, consisting of excessively tuberculated floral parts inhabited by an eriophid mite." The mite is similar to or possibly the same as *E. rhodiolae* Can. of *Rhodiola rosea* of Central Europe and Italy.

There is also a paper by SEARS[13] in which the author records, figures, and gives a brief description of 63 species of galls from Cedar Point, Ohio. These galls occur on 31 species of host plants representing 22 genera.

An important taxonomic paper has been published by FELT,[14] in which he lists a number of species and also describes several new species. This paper will enable the botanist to determine a large number of species which come to their attention.

WELLS[15] describes 22 unreported galls from Connecticut and figures many of them.

FULLWAY[16] gives a very interesting taxonomic paper on gall fly parasite as a result of studies on Mrs. ROSE PATERSON BLAKEMAN'S collection in Uni-

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[11] HEALD, F. D., Aerial galls of the mesquite. Mycologia **4**:37–38. *pl. 1*. 1914.

[12] COCKERELL, T. D. A., A mite gall on *Clementsia*. Entomol. News **25**:466. 1914.

[13] SEARS, PAUL B., The insect galls of Cedar Point and vicinity. Ohio Nat. **15**: 377–388. *pls. 4*. 1914.

[14] FELT, E. P., Additions to the gall midge fauna of New England. Psyche **21**: 109–114. 1914.

[15] WELLS, B. W., Some unreported cecidia from Connecticut. Ohio Nat. **14**: 289–296. *pls. 2*. 1914.

[16] FULLWAY, D. T., Gall fly parasite from California. Jour. N.Y. Ent. Soc. **20**: 274–282. 1914.

versity of California. Although this paper deals primarily with taxonomic entomology, it contains many good records of American cecidia.—MEL. T. COOK.

Cytology of the Uredineae.—FROMME[17] described the processes taking place in the formation of spores of the flax rust, *Melampsora Lini* (Pers.) Desm., thereby adding another rust to those whose life histories are fully known. Interest in these forms centers largely in the aecidium. The sequence of events here is essentially like that occurring in aecidia of the *Caeoma* type, and differs only in the formation of two sterile cells above each gamete instead of one. The gametes fuse laterally in pairs, but occasionally irregularities apparently occur, since basal cells with three or more nuclei are found; however, the mode of origin of these cells was not observed. The spermatiophores also differ from the usual form. They are divided into a number of cells, from each of which a branch bearing a spermatium arises. The spermagonia and aecidia are closely associated and mature at the same time. The formation of the uredospores and teleutospores offers nothing noteworthy.

In a preliminary paper, KUNKEL[18] gave an account of the highly interesting discovery that the widely distributed blackberry rust (*Caeoma nitens*), which as a result of infection experiments reported by TRANZSCHEL and by CLINTON has been regarded as the aecidial form of *Puccinia Peckiana*, is in reality a rust of the *Endophyllum* type. This conclusion was based upon the observation that the aecidiospores of this fungus on germination produce promycelia and sporidia instead of germ tubes. The nuclear phenomena occurring in the germinating aecidiospores are described in a later paper. The processes agree in all essentials with those in the germinating aecidiospore of *Endophyllum*. At maturity the aecidiospore contains two nuclei; these fuse just before germination begins, so that the germinating aecidiospore is uninucleate. The fusion nucleus usually passes into the germ tube, where two divisions take place. Occasionally the first division occurs in the spore, as also reported by HOFFMAN[19] for *Endophyllum Sempervivi*. Five cells are formed in the promycelium; the basal one, which is continuous with the spore cavity, contains very little protoplasm and no nucleus. Sporidia arise from the other cells in the usual way. At the time of germination the sporidia are generally binucleate as a result of a division of the original nucleus. These facts, as well as some negative results of infection experiments on blackberry leaves reported by the author, leave no doubt that the orange rust of the blackberry is an independent fungus of the type of *Endophyllum*, and not, as hitherto supposed, the aecidial generation of

[17] FROMME, F. D., Sexual fusions and spore development of the flax rust. Bull. Torr. Bot. Club **39**:113–129. *pls. 2.* 1912.

[18] KUNKEL, L. O., The production of promycelium by the aecidiospores of *Caeoma nitens* Burrill. Bull. Torr. Bot. Club **40**:361–366. *fig. 1.* 1913.

———, Nuclear behavior in the promycelium of *Caeoma nitens* Burrill and *Puccinia Peckiana* Howe. Am. Jour. Bot. **1**:37–47. *pl. 1.* 1914.

[19] Rev. BOT. GAZ. **54**:437. 1912.

Puccinia Peckiana. The wonder is that the true nature of this exceedingly common fungus has remained so long unknown.

Although the cell rows forming the peridial wall in the aecidia of rusts are usually considered to be homologous with the spore chains, it is generally believed that the intercalary cells characteristic of the spore chains are not produced in the peridium. This belief is shown to be erroneous by KURSSANOW,[20] who has investigated the development of the aecidia of a number of rusts belonging to different types, and finds that a complete homology exists between the spore chains and the cell rows of the peridium. The basal cells of the peridium give rise to a series of cells each of which divides, cutting off a small cell in the lower outer corner. These small cells, which correspond to the intercalary cells, thus lie toward the outside of the rows of cells forming the peridial wall. By the growth of the peridial cells they are crowded still farther outward. Their walls soon become gelatinous and their structure is entirely obliterated, so that they are not recognizable in any but the youngest parts of the aecidium. This fate undoubtedly accounts for their remaining long unobserved. The basal cells of the peridial rows, as well as the peridial cells themselves and the "intercalary" cells, are binucleate, like the basal cells of the spore chains, in all cases except in a form of *Aecidium punctatum* Pers. (*Puccinia Pruni-spinosae* Pers.), which had uninucleate basal cells, giving rise to uninucleate aecidiospores and peridial cells. This form resembles in this respect the aecidium with uninucleate cells which was described by MOREAU[21] on *Euphorbia silvatica*. This form, which was the first aecidium having uninucleate cells discovered, has uninucleate basal cells also.—H. HASSELBRING.

Resistance of mosses to drought and cold.—It has long been known that many mosses are able to survive long periods of extreme drought, although much of this knowledge has been based upon inexact and insufficient data. It is a matter of much satisfaction, therefore, to have their resistance proved by the exact and extensive results of IRMSCHER[22] based upon careful experiments with a large range of species. He finds resistance to drying related in a remarkable degree to the ordinary habitat of the species, but often surprisingly great even for aquatic and mesophytic forms. Thus, while the leaves of some species of *Fontinalis* succumbed to a week's air-drying or to five days in a desiccator, *Philonotis fontana* and three species of *Hypnum* died only after 15–20 weeks in dry air or 10–18 weeks in the desiccator. The mesophytic species from deciduous forests, like *Barbula subulata, Bryum alpinum, Mnium*

[20] KURSSANOW, L., Über die Peridienentwicklung im Aecidium. Ber. Deutsch. Bot. Gesells. **32**:317–327. *pl. 1.* 1914.

[21] MOREAU, Mrs. F., Sur l'existence d'une forme écidienne uninucléeé. Bull. Soc. Mycol. France **27**:489–492. *fig. 1.* 1911.

[22] IRMSCHER, E., Über die resistenz der Laubmoose gegen Austrocknung und Kält. Jahrb. Wiss. Bot. **50**:387–449. 1912.

rostratum, and *Catherinea undulata*, withstood 8–20 weeks in dry air and 5–13 weeks in the desiccator; and in species from the drier coniferous woods, like *Bartramia ityphylla*, *Dicranum fuscescens*, and *D. scoparium*, the resistance to dry air extended to 50 weeks and in the desiccator to 40 weeks. It should be noted that after all the leaves were thus killed, many of the plants were still capable of growth by fresh shoots from the more resistant stems.

The leaves of soil-growing mosses showed drought resistance in the desiccator for periods varying from 3 weeks for *Physcomitrium pyriforme*, 7 weeks for *Bryum pallens*, and 9 weeks for *Funaria hygrometrica*, to 15 weeks for *Bryum argenteum* and 35 weeks for *Pottia lanceolata*. Rock-inhabiting species included remarkably resistant forms, as *Grimmia pulvinata*, enduring 60 weeks of exposure in the desiccator, and *Schistidium apocarpum*, with one-fourth of it leaf cells still alive after 128 weeks of continuous exposure to dry air. Species from tree trunks showed resistance of the same order as those from the rocks.

Field observations show that alternate wetting and rapid drying are more detrimental in their effects than continuous drought; also, that the same species growing under different conditions varies in its drought resistance, the more hardy being that from drier habitats. Protonemata were correspondingly variable, withstanding the action of the desiccator for periods varying from 2 weeks for the cultivated protonema of *Funaria hygrometrica*, whose natural grown protonema was twice as resistant, to 14 and 15 weeks for the natural growing protonema of *Bryum argenteum* and *Catharinea undulata*. The sporophytes were found to be almost as hardy as the protonemata.

Experimental evidence from a large number of species points to great uniformity in the power of resisting cold. At $-20°$ C. the leaves of most species were killed, whether they came from the aquatic, mesophytic, or xerophytic habitats, although in a few species the resistance extended to $-30°$ C. Plants grown at a high temperature showed decidedly less resistance to sudden cold than those from cooler situations. Attention is also directed to the relation between turgor and the freezing point of the leaf cells, and to the behavior of the cells in various solutions.

The evidences of careful methods, the large number of species under experimentation, the abundance of the quantitative data, and the logical organization of the report are to be commended.—GEO. D. FULLER.

Composition and qualities of coal.—By improved methods of making thin coal sections, JEFFREY[23] claims to have obtained new and valuable results in the study of the composition of coal. In a brief discussion of the modes of accumulation he holds the view that at present both the accumulation as the result of heaping up of the remains of successive generations of plants in peat bogs, the autocthonous hypothesis, and the accumulation by drift in lakes or lagoons, the allocthonous hypothesis, commonly take place in the temperate

[23] JEFFREY, EDWARD C., On the composition and qualities of coal. Econ. Geol. 9:730–742. 1915.

climates; but that only the method of accumulation by drift takes place in the tropical climates, nothing corresponding to a peat bog of the temperate climates having been found in the tropics. As the result of microscopic examination of coal he concludes that the allocthonous hypothesis also harmonizes best with the structure of coal. This is in harmony with the view of some that the climate of the coal-forming epochs approached present tropical conditions.

The canneloid coals, such as the true cannel coals, tasmanite, and boghead coals, are composed chiefly of spores, now very much crinkled and collapsed, imbedded together with some woody material in a dark ground substance. Hitherto many of these bodies had been considered gelatinous algae, but owing to improved methods can now be identified as spores. These coals have been formed under open water, representing the muck of ancient lakes or lagoons.

The ordinary bituminous coals are composed of both woody or lignitoid material and spore or canneloid matter in varying proportions. The woody or lignitoid constituent, known in descriptive terminology as glanz coal, is found in layers and has lost completely its original organization, a condition generally observed in coals derived from vegetable débris. Carbonized wood or charcoal is the only material derived from the grosser parts of plant bodies which retain structure in coal. Between the shiny woody or lignitoid layers are lodged the duller canneloid layers, known in descriptive terminology as matt coal, consisting of a dark ground substance in which are imbedded remains of flattened spores.

Coals, therefore, may be composed of three recognizable constituents: (1) spores or canneloid, (2) modified wood or lignitoid, and (3) less commonly relatively unmodified carbonized wood or charcoal. The properties of coal, he conjectures, depend to a very large degree upon the proportions of the original constituents; coals rich in spores, such as cannels, bogheads, and oil shales, are highly bituminous, and in some form or other are the mother substance of oil and gas. The spore contents of a coal determine the fatness, and in all probability have a definite relation to its coking properties; the lignitoid constituent, on the other hand, reduces the bituminosity and coking value of coal.—REINHARDT THIESSEN.

Self-sterility in Nicotiana.—EAST[24] has studied self-sterility in hybrids between *Nicotiana forgetiana* (Hort.) Sand. and *N. alata* Lk. and Otto. var. *grandiflora* Comes. The parent plants were both self-sterile, though self-fertile plants occur in at least one of the parent species. All the hybrids tested (over 500 plants of F_1, F_2, F_3, and F_4) were self-sterile. The F_1 plants, like the parent species, had 90–100 per cent of morphologically perfect pollen, except for a single plant with only 2 per cent of good pollen. Cross-pollination between individual plants of F_2, F_3, and F_4 demonstrated a high degree of cross-fertility. There was found 1.5 per cent of apparent cross-sterility in F_2, 6 per

[24] EAST, E. M., The phenomenon of self-sterility. Amer. Nat. **49**:77–87. 1915.

cent in F_3, and 9 per cent in F_4. Back crosses with the parents resulted in about 6 per cent of cross-sterility.

Self-sterility was found to be wholly a matter of rate of growth of the pollen tubes. The pollen germinated perfectly on stigmas of the same plants, but the pollen tubes grew at the rate of about 3 mm. per day, and in no case traversed over half the distance to the ovary in the 11 days maximum life of the flowers. Growth of pollen tubes in cross-pollinated styles, on the other hand, though starting at about the same rate, was so continuously accelerated that the ovaries were reached in 4 days or less.

The simple Mendelian explanations of self-sterility proposed by CORRENS for *Cardamine pratensis* and by COMPTON for *Reseda odorata*[25] are not applicable to self-sterility in *Nicotiana*. EAST suggests that specific "Individualstoffe" of the nature of enzymes present in the pollen grains can, except in the plant that produced the pollen and in other plants of like germinal constitution, "call forth the secretion of sugar that gives the direct stimulus" to growth of the pollen tube. The hypothesis satisfies the facts presented as regards both the total self-sterility in all generations and the slight cross-sterility, which increases from generation to generation as the percentage of plants of like germinal constitution increases. It occurs to the reviewer that, if the pollen or pollen tubes have specific abilities to call forth the growth stimulus in plants of unlike germinal constitution, while the stimulus itself (the secreted sugar perhaps) is not specific, simultaneous cross and self-pollination of the same flower should result in at least partial self-fertility. Evidence derived from such pollinations would in any case be of interest.—R. A. EMERSON.

Cytology of the Mucors.—Miss KEENE[26] has given an account of the development of the zygospores of *Sporodinia grandis*. She finds at first no essential morphological difference in the two sexual branches which give rise to the zygospore. Slight differences in size are not regarded as significant. Later the branches differ somewhat in their internal structure. The protoplasm of one branch is retracted from the cell wall, the intervening space being filled with a granular substance. Sometimes there is a slight retraction of the protoplasm of the opposite branch also. The nuclei in the sexual branches are small and have the same structure as the nuclei in the rest of the mycelium. They appear to increase in number, but divisions were not observed. When the sexual branches meet, their walls coalesce in the region of contact. At this time a portion of the protoplasm in the end of each branch is delimited either by cleavage furrows which cut in from the walls, or by vacuoles which enlarge and cut through the protoplasm to the hyphal wall. In either case a central strand remains connecting the protoplasm of the suspensors with that of the gametangium. Walls cutting off the gametangium from the suspensors grow in

[25] BOT. GAZ. **57**:242–245. 1914.

[26] KEENE, Miss M. L., Cytological studies of the zygospores of *Sporodinia grandis*. Ann. Botany **28**:455–470. *pls. 2*. 1914.

from the hyphal wall. The opening is finally completely closed, and at the center an excess of material is deposited, giving rise to a papilla-like structure described as a "canal" by LÉGER. During this process the wall between the two gametangia is resorbed. The line of contact between the two protoplasmic bodies remains distinct for some time, owing to the presence between them of the granular material mentioned above, but finally the masses fuse. Multiple nuclear fusions appear to occur at this stage. The nuclei which fail to fuse are smaller than the fusion nuclei, and soon disintegrate. No evidence of a uninucleate stage was observed. At this time numerous oil bodies, which are regarded as being of the same nature as the elaioplasts of higher plants, appear in the protoplasm. These bodies fuse until two or three large ones are formed. The large elaioplast-like bodies the author believes to be the "sphère embryonaires" of LÉGER.—H. HASSELBRING.

Proceedings of the National Academy.—This new monthly publication began to appear with the January issue of 1915. In addition to the reports and announcements that belong to it naturally, as the official organ of publication of the National Academy, it will also serve as a medium for the prompt publication of brief original papers by members of the Academy and other American investigators. The papers will be much shorter and less detailed than those published in the special journals, and the aim of the *Proceedings* is to secure promptness of publication and wide circulation of the results of American research among foreign investigators. The editorial board includes a representative from each one of the special fields of science, the editor of the BOTANICAL GAZETTE being the botanical representative on the editorial board of the *Proceedings*.

The first two numbers contain the following botanical papers: *Phoradendron*, by WILLIAM TRELEASE (Proc. Nat. Acad. **1**:30–35. 1915); The morphology and relationships of *Podomitrium malaccense*, by DOUGLAS H. CAMPBELL (*ibid.*, 36, 37); and A phylogenetic study of cycads, by CHARLES J. CHAMBERLAIN (*ibid.* 86–90). In addition to these papers that are credited to the section of botany, certain papers in genetics, physiology, and chemistry come well within the scope of present botanical interest. For example, the paper by E. M. EAST, entitled An interpretation of self-sterility (*ibid.* 95–100), deals with an interesting problem of genetics among plants.—J. M. C.

Evolution of the flower.—HORNE[27] has contributed a very detailed study of the structures of the flower which he regards as indicators of phylogeny. The families specially studied are the Hamamelidaceae, Caprifoliaceae, and Cornaceae, but the principles involved have general application. He includes in his discussion also the possible applications of the various theories of evolu-

[27] HORNE, A. S., A contribution to the study of the evolution of the flower, with special reference to the Hamamelidaceae, Caprifoliaceae, and Cornaceae. Trans. Linn. Soc. London II Bot. **8**:239–309. *pls. 28–30. figs. 13.* 1914.

tion, and especially the light shed upon his problem by the work in plant genetics. Much stress is laid upon the varying characters of the ovule and its connections, beginning with the orthotropous ovule, as relatively primitive, and advancing through "anatropal advance and specialization," which latter, by the way, is said to be accompanied by the transition from two integuments to one. It is interesting to note that in the author's judgment "no phylogenetic significance can be attached to a particular form of vascular system." Applying his criteria, HORNE concludes that both Caprifoliaceae and Cornaceae are polyphyletic, and warns the authors of "systems" that "knowledge of the phylogeny of angiosperms can only be truly advanced by the detailed morphological and experimental investigation of many more families, and then, but not till then, can ENGLER's system be replaced by a greater scheme, more nearly approximating to natural relationships."—J. M. C.

Fossil plants from Kentucky.—Six genera of fossil plants from Kentucky are the subject of an intensive study by SCOTT and JEFFREY.[28] The exact level from which these plants come is somewhat in doubt, but the evidence favors the base of the Carboniferous, although the uppermost Devonian is not excluded. The fossils belong to three groups. The first of these includes the stem of one of the Cycadofilicales known as *Calamostachys* with its petiole (*Kalymma*), another petiole of the related genus *Calamopteris*, and a petiole referred to the genus *Periastron*. All of these genera have previously been known only from the Culm of Germany, where they were found and named by UNGER, and it is of much interest to note that the same flora existed on this continent, although as far as is known the species were distinct. The second group comprises two new genera: *Stereopteris*, which is apparently the petiole of a fern, and *Archaeopitys*, which presents a new and interesting type of cordaitean stem. In a third group may be placed a cone of the usual *Lepidostrobus* type. The structure is fairly well preserved in most of these fossils, and is illustrated in the 13 quarto plates which accompany the paper.—M. A. CHRYSLER.

Phylogeny of angiosperms.—In continuing their studies of this subject, SINNOTT and BAILEY[29] have investigated the evidence to be obtained from leaves. They conclude, from paleobotanical evidence, from the correlation between the palmate leaf and the multilacunar node, and from the frequency of this type of leaf in the relatively primitive groups, that the leaf of the primitive angiosperm was palmate in type and probably lobed. They

[28] SCOTT, D. H., and JEFFREY, E. C., On fossil plants showing structure, from the base of the Waverley Shale of Kentucky. Trans. Roy. Soc. London **205**:315–373. *pls. 27–39.* 1914.

[29] SINNOTT, E. W., and BAILEY, I. W., Investigations on the phylogeny of the angiosperms. 5. Foliar evidence as to the ancestry and early climatic environment of the angiosperms. Amer. Jour. Bot. **2**:1–22. *pls. 1–4.* 1915.

think that the chief factor in the evolution of the now dominant pinnate type of leaf to have been the development of the petiole. Since among woody plants the more ancient multilacunar type of node predominates in temperate regions, and the palmately lobed leaf among such plants is also almost entirely confined to temperate regions, they infer that angiosperms first appeared under climatic conditions more temperate than tropical, a climate in the Mesozoic probably found only in the uplands. Furthermore, such evidence from leaves indicates that the angiosperms have come from the palmate coniferous stock, rather than from the pinnate cycadean stock; and also that the monocotyledons were derived from some ancient palmate group of the dicotyledons.—J. M. C.

Seeds of Polygonaceae.—WOODCOCK[30] has investigated the seeds of representative genera of Polygonaceae, and has reached some interesting conclusions. He finds that the outermost layer of the nucellus becomes transformed into a nutritive jacket before fertilization, and that this layer apparently carries food material from the chalazal region to the developing endosperm. He also describes the growth of the endosperm by the activity of a "cambium-like layer," which is differentiated very soon after cell formation begins, and also calls attention to the varying position of the embryo in reference to the other structures of the seed. He concludes that in the germination of certain of the seeds which were investigated from this standpoint, the aleurone layer has a digestive function, secreting a ferment which converts the starch of the endosperm into available form for translocation. In some cases also the absorbed carbohydrate is temporarily reconverted into starch in the tissues of the embryo, the cotyledons being the principal storage region.—J. M. C.

Fossil cycads.—Miss HOLDEN[31] has studied the relationship between *Cycadites* and *Pseudocycas*. The latter genus was established by NATHORST for certain cycad-like leaves formerly referred to *Cycadites*, but differing from the latter genus in having a double instead of a single midrib, and in the fact that the pinnules are not narrowed, but if anything broaden at the point of attachment to the rachis. Miss HOLDEN reaches the interesting conclusion that *Pseudocycas* belongs to Bennettitales, as judged by the character of the stomata and the epidermal cells. She further concludes that the presence of a double or single midrib is of no diagnostic importance, and that the name *Pseudocycas* should be applied only to leaves whose cuticular structure is known.—J. M. C.

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[30] WOODCOCK, E. F., Observations on the development and germination of the seed in certain Polygonaceae. Am. Jour. Bot. 1:454–476. *pls. 45–48.* 1914.

[31] HOLDEN, RUTH, On the relation between *Cycadites* and *Pseudocycas*. New Phytol. 13:334–340. *pl. 3. fig. 1.* 1914.